Concepts Unbound

A Norm-Based Account of the Justification of Conceptual Analyses

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Abstract

In this thesis, I argue that philosophical methodology is lead astray by focusing on intuitions. The supposition that philosophers rely on intuitions for defending and attacking theories—conceptual analyses in particular—has no methodological significance. Accordingly, I reject the requirement that conceptual analyses have to support and stand in support from intuitions. I articulate and defend a handful of norms in its place. Most conspicuously, I defend norms that I claim have a purely rational and pragmatic grip on conceptual-analytic practice. The norms are conditions on being a good analysis. The resulting notion of a good analysis cannot be equivocated with the notion of an analysis that expresses a conceptual truth.

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1. Introduction

There is an ongoing debate about the method we do and should employ in analytic philosophy. The primary issue concerns what intuitions are and whether they play a role in justifying philosophical theories.[1] Are intuitions evidence, or sources of evidence, for the kind of claims we aim to establish in philosophy?[2]

We can frame the debate as a matter of attacking or defending the use of intuition as such, but there are other prevalent issues as well. For one thing, the dispute over intuition use is conjoined with questions about the adequacy of viewing contemporary analytic philosophy as engaged in conceptual analysis, and about the viability of conceptual analysis as such (Williamson 2007). This conjunction means there is critical reflection, not only on the means we employ in analytic-philosophical practice, but also on the ends we set ourselves by engaging in that practice. We inquire to know how we achieve philosophical knowledge and understanding, and what that knowledge and understanding consist in. Moreover, having a theory of both the means and ends of philosophy is paramount to having a theory of philosophy. Thus, we can correctly conceive of the methodological enterprise as metaphilosophy or as the philosophy of philosophy.

To provide an overview, here is a sample of questions that are at the forefront of current metaphilosophy. Do we (analytic philosophers) use intuitions as evidence, or sources of evidence, when performing conceptual analysis? If intuitions are basic evidence, is it possible to attack their use? (Bealer 1996) Should intuition appeals be blamed for their fallibility, or for their non-satisfaction of other epistemic desiderata, such as error-detectability? (Weinberg 2007) Is conceptual analysis really the primary analytic-philosophical activity, or is that a misconception due to a lack of articulate method? (Williamson 2007) If philosophers do rely on intuitions, should we not probe the intuitions of non-philosophers? (Weinberg, Nichols and Stich 2001) Do the results

[1] The use of the word "intuition" in the context of this thesis should not be confused with its use in translations of Kant, where it is an awkward substitute for the German word "Anschauung". The exception is p. 57 of this thesis. Cf. footnote 53.

[2] The debate is fairly young. Although there are predecessors—inevitably, since the issue is continuous with first order philosophical issues—the articles in DePaul and Ramsey (eds.) (1998) seem to me as a definite starting point of a systematic metaphilosophical intuition debate.

from cognitive-psychological research have implications for how we ought to proceed in philosophy? How should we explain the lack of agreed-upon analyses and, more generally, the overwhelming amount of disagreement in philosophy? Does philosophy seek *a priori* knowledge? More fundamentally, is knowledge the aim of philosophy, or do we rather aim at something like understanding? (Elgin 2006) Can we discern any real progress in philosophy, save the development of modern logic? Will scientific progress render philosophy obsolete?

In order to not get lost in all of these questions, we must keep sight of the basic narrative. Analytic philosophy was seen as relying on intuitions for arriving at non-empirical (conceptual or necessary or *a priori*) truths. In turn, this reliance was challenged by experimental philosophers, and more fundamentally from the hold of naturalists and philosophers updated on results in cognitive psychology (e.g. Kornblith 1998; Kahneman, Slovic and Tversky 1982)[3]. Most of the other questions emerge from this challenge and the self-consciousness it has provoked.

In section 1.1 I articulate my thesis statement and elaborate the course of argument. Then, in section 1.2, I preview my positive account in more detail. In sections 1.3 and 1.4 I declare and reason for my approach to methodology.

1.1 Thesis statement and chapter division

The question I attempt to answer in this thesis is:

> *How should conceptual analyses be justified?*

The relevance of this question is a contested issue. Not everyone thinks conceptual analysis is viable.[4] Holding conceptual analysis as a (if not the) primary philosophical activity is yet more controversial. However, the issue of the relevance of conceptual analysis cannot be properly resolved without a conception of what it is. I believe that

[3] The latter of these is a collection of essays in psychology. Although they do not concern philosophical intuitions in particular, we can easily draw implications for the epistemic well-being of philosophy, provided that philosophers do rely on intuitions and that there is no principle difference between philosophers' intuitions and the ones reported in the essays.

[4] Williamson (2007) is the current most pressing opponent in this regard. The classical challenge to the idea of analyticity, and to conceptual theorizing in extension, is due to Quine (1951).

if such a conception is worked out—which is what this thesis is a step towards—the issue more or less solves itself, but I leave it to the reader to judge this.

The most important claims in this thesis are summed up below, in (A), (B) and (C).

(A) Intuitions are propositional attitudes that are defined by a dialectical role (= the intuiter does not have positive argument or evidence for holding the proposition in the context of holding it as an intuition), and by the unsettled strength of assent they involve. Moreover, having an intuition that p is not evidence that p is the case. The fact that p can be evidence for other propositions while being the content of an intuition has no methodological import.

(B) Conceptual analyses are justified by satisfying a handful of norms. Norms that have a purely rational and pragmatic grip on analyses comprise a significant sub-group of these, and their satisfaction is an independent issue from how well the (pre-analytical) content of the analyzed concept is reflected by the analysis.[5]

(C) Following the norms of (B), there are no conceptual truths beyond good analyses. When we analyze concepts, we should treat them as targets of rational assessment and configuration, not as psychologically real entities, nor as prefixed members of an *a priori* structure. The concerns of those who employ the analyzed concept are also our concerns as analysts of that concept, and this has implications for what norms we can hold conceptual analyses answerable to.

The arguments for (A), (B) and (C) make up the chapters beyond this introduction. The structure of the argument is as follows. To assess how conceptual analyses should be justified, I must first develop an account of what has been understood as their primary justificatory source, namely intuition. Hence, the second chapter is concerned with what intuitions are and whether they have status as evidence. I argue that intuitions are cognitive pro-attitudes whose content the intuiter has no positive justification for holding in the dialectical context she is situated in when having that

[5] Instead of speaking of what contents the concepts we analyze *really* have, I employ the expressions "pre-analytical concept" and "pre-analytical content of a concept" to denote the concept or its content as they figure in absence of the analysis. It is necessary to use such expressions in case it is possible—as I shall argue—that analyses can legitimately purport to revise the content of a concept. In that case, it is obscure to consider how well an analysis captures the content a concept *really* has.

intuition. Furthermore, the strength of assent involved in this attitude is unsettled. That is, we do not know how strongly A is committed to p, by knowing that A has sincerely uttered "Intuitively, p." I then argue that we cannot attribute an evidential status to intuitions.

Denying that intuitions are evidence leaves us with a justificatory void. The third chapter attempts to fill this void with the norms I articulate and defend. The rational and pragmatic norms are those for *internal compatibility, external compatibility, understandability, conceptual integration, determinacy, and explanatory and expressive utility*. Norms of this kind have not been sufficiently acknowledged, and they have gone completely missing in the debate about the role of intuitions. There is also an empirical norm. It demands that the pre-analytical content of a concept is conserved by the analysis. The latter is not, however, accorded the super-ordinate status it usually is. Instead, I attempt to make room for the possibility of not capturing the pre-analytical content of the analyzed concept perfectly, without failing as an analysis. The account I defend has room for conceptual revision. Revision is legitimate when the pre-analytical content is itself not rationally and pragmatically optimal.[6]

The rational and pragmatic norms are only reasonable from the point of view of being a participant of the practice of employing the analyzed concepts. We have no incentive to make sure our concepts are shaped and ordered in rational and pragmatic ways unless we have a purpose of employing those concepts. If we were not employing the concepts we analyze, it would not matter to us if, for instance, the content of a concept were internally inconsistent. The fourth and last chapter argues that the point of view of being a participant is reasonable and philosophically worthwhile. It contrasts this with what the case would be if the empirical norm were

6 On more usual accounts of conceptual analysis, it is understood as a purely conservative enterprise. As Lewis claims, "One comes to philosophy already endowed with a stock of opinions. It is not the business of philosophy either to undermine or justify these pre-existing opinions, to any great extent, but only to try to discover ways of expanding them into an orderly system." (Lewis 1973b: 88).

Since my view diverges from this quite a bit, some might find it awkward that I claim to be giving an account of the methodology of conceptual analysis. However, I would have no problem with a different label for the activity I am prescribing norms for. If the reader finds another expression more fitting (e.g. "conceptual synthesis" or "philosophical theories of concept contents") I urge him or her to think of this project in terms of that. Cf. section 4.2.

the only norm analyses were answerable to, and with a psychological point of view on what concepts are. On premises of the view I defend, we aim at mastering concepts by explicating them in a disciplined manner and revising them when needed.

1.2 Preview of the account

In summary, my main and distinctive positive claim is that, as the masters of concepts, the claims we make about their contents ought to accord with some norms that are rational and pragmatic in kind. I want to clarify what I am intending by this norm-talk so the reader can see where the argument is heading before the norms are explicated in detail.

A premise is that, when we make claims about concept contents, we are committing ourselves to a stance on how it is *correct* to apply the concept in question. This kind of claim is patently normative. It purports to govern the use of the expression(s) that have the target concept as their content.[7] Consequently, we should not require that every actual application of the relevant concept is reflected in the analysis we promote. After all, some applications are wrong, and concepts are sometimes applied in incompatible ways. Empirical psychologists, on the other hand, are obliged to take any fact about the actual application of a concept, or the intuition that supports that application, as an empirical datum (Rosch and Mervis (1998) is an example). So empirical-psychological theories about concepts must answer to the norm that they should be *representationally* correct; they should say empirically true things about the mental states or structures that count as concepts in the psychological sense. While there are, no doubt, other norms these theories answer to, the important thing to notice in this context is the contrast with philosophy as to the authority of this particular norm.

In the justification of conceptual analyses, the same norm—the demand to represent/reflect *de facto* concept use and intuition—has a conspicuously subordinate

[7] I shall sometimes speak of *using* concepts. This should be understood as elliptical talk of using the expressions that have those concepts as their contents. This manner of speech is innocent. At any rate, we cannot single out a concept without making use of an expression of it, or a content-conveying description of it. This is evident in one of the standardized ways of making reference to a concept—which I have adopted for this thesis—by putting the relevant expression in small caps, thus: JUSTICE, NEGATION and PET FISH.

position. First, these *de facto* applications (and intuitions to the same effect) are not even strict evidence of what the pre-analytical content of a concept is. There are no collections of descriptive facts from which we can ascertain the content of a concept. That is, there is no simple or complex function we could apply, that takes those facts as arguments and gives concept contents as values (unless we already assume the distinction between correct and incorrect concept applications, or some equivalent).[8] Second, even if we are able to access the pre-analytical content of a concept (which we are), there is no guarantee that the pre-analytical concept is optimized for the explanatory, expressive and representational work it is being set to do. Unless we are analyzing a concept just to show why it is a bad concept—i.e. a concept we should discard—analyses should be normatively constrained by the same norms that constrain concept formation in general, even if that would involve not capturing the pre-analytical content perfectly.[9] To make sense of these claims, we need to assess specific candidates for such norms. In the following, I sketch three of the rational and pragmatic norms I articulate and defend later on.

The norm Internal Compatibility (section 3.1.1) prohibits inconsistency in a concept by requiring that the applicability conditions an analysis ascribes to it are compatible with each other. There is no psychological guarantee against inconsistency, nor a social guarantee against it, so we cannot conceive of this norm as representing psychological actuality, nor conceive of it as being built into the pre-analytical contents of concepts. Rather, the norm is substantiated by rational considerations, if anything.

Understandability (section 3.1.3) is another rational and pragmatic norm. It says that analyses ought to advance our understanding of the concept in question. In the light of a specific picture of conceptual analysis—on which capturing the contents of concepts is all conceptual analysis is about—we might question the reasonability of such a demand. However, there are argumentative patterns we are and should be committed to, that seem hard to make sense of without a foundation from a norm of this sort. For

[8] Cf. section 3.2.

[9] To be sure, it is sometimes valuable to prioritize satisfying the norm to capture the pre-analytical content of a concept at the expense of not satisfying the rational and pragmatic norms, but in that case we do not intend to continue employing the analyzed concept. Cf. section 4.1.

instance, it seems impossible to explain why most circular analyses are bad, without a norm that has some connection to our goal of making things understandable.[10] This gives it some plausibility, at least in the absence of other norms that could explain this. In particular, the aforementioned empirical norm is no foundation for this argumentative pattern.

A third rational and pragmatic norm is Determinacy (section 3.1.5). It requires that the conditions of applicability (that an analysis ascribes to a concept) be determinate. It is of no help to have a concept unless we can apply it, and indeterminacy hinders our ability to do so.[11] If it is a good thing that concepts are determinate, then conceptual analyses ought to provide determinate conditions of applicability. To see the reasonability of this norm, consider its non-satisfaction. For instance, consider an analysis that ascribes to TRUTH the applicability condition that the candidate truth-bearer must uncover (something).[12] It is not determinate what we should count as uncovering (something), so this is a blameworthy feature of the analysis in consideration. Another example is David Lewis' criterion for assessing counterfactuals, as stated in "Causation" (1973a: 560). When assessing the non-vacuous truth of a counterfactual, we must judge whether there is a possible world, in which both the antecedent and consequent are true, *that is closer to the actual world* than any possible world where the antecedent is true but the consequent false. In other words, "a counterfactual is nonvacuously true iff it takes *less of a departure from actuality* to make the consequent true along with the antecedent than it does to make the antecedent true without the consequent." (Lewis 1973a: 560, my emphasis). However, it is not determinate what it is for a world w_1 to be closer to world w_2 than

[10] The norm I really take to explain why most circular analyses are bad, is the norm for conceptual integration (section 3.1.4), but it is not easily explicated, and as I conceive of that norm, it subserves the norm for understandability.

[11] Determinacy comes in degrees. I am not claiming that the slightest amount of indeterminacy makes a concept useless. The existence of vague but useful concepts, such as BALD, TALL and DRY, proves this much. For these concepts, it is most often not practically necessary to sharpen their determinacy, and that might in turn explain why they are not apt for analysis, on the premises of my account. Our interest in sharply distinguishing instances from non-instances is more apparent in the case of concepts such as KNOWLEDGE, since their applicability or no makes a huge practical difference to us. See section 3.1.5.

[12] This is related to Heidegger's conception of truth, as expressed in *Being and Time* (1962: §44). I am not making an argument that this particular weakness of Heidegger's analysis is a knockdown reason to dismiss it. It can be made up for in other ways—but I am no Heidegger scholar.

world w_3 is to w_2. How do we compare incommensurable differences between worlds, such as differences in humidity versus differences in light conditions? Lewis notes the vagueness of the overall similarity relation between worlds that he is relying on, but he takes it to be non-critical for the goodness of his analysis (1973a: 559-560). It might be non-critical, but it is a weakness we should take seriously. If it is possible to achieve a more determinate analysis of CAUSATION that is equal in other regards of goodness, such an analysis would be preferable.[13] As with the norm that analyses ought to benefit our understanding of the target concept, the norm against indeterminate conditions of applicability is not guaranteed to be satisfied by satisfying the empirical norm. An analysis can ascribe indeterminate conditions to the applicability of a concept that still match up with the pre-analytical content of that concept.

I will elaborate and argue for these norms together with the norms not sketched here—external compatibility, conceptual integration, and expressive and explanatory utility—in chapter three. I make no claim to completeness, and the norms are not wholly independent from each other. (For instance, an analysis' having determinate conditions of applicability is in part what makes for its explanatory utility.)

1.3 A normative approach to methodology

A methodologist should clarify whether his main purpose has a normative dimension or not. Does the project aim to represent past or current philosophy by correctly describing it, or does it aim for a prescription of how philosophy ought to be done? We can have an interest in both, but strictly speaking, they are different tasks.

The difference between the normative task and the non-normative descriptive task can be fleshed out in how they constrain argumentation. Correctly describing a practice requires for its justification careful study and systematization of the actual performances within that practice. For philosophy, this means exegetical work on a representative sample of texts within the kind of philosophy one is theorizing about.[14] On the other hand, correctly prescribing a practice—i.e. making a claim to correctness

[13] The indeterminacy of Lewis' analysis of causation receives a more detailed treatment in the section on determinacy (3.1.5).

[14] Cappelen (2012) is a good example.

for some specified practice—requires for its justification that the virtues of going about in the specified way are established by argument. For philosophy, this means sorting out what kinds of virtues its method is to be judged according to,[15] and articulating norms that express those virtues.

The tasks are not always kept separate. Methodological claims can be implicitly normative. Consider a title for an imaginary introductory book in mathematical methodology, *How Mathematics is Done*. On the basis of its title, it would be far-fetched to expect the book not to make any claim as to how math is correctly performed. We can make this point vivid by seeing how our expectations would be broken if what the book really were concerned with, were how averagely skilled five-year-olds perform addition and subtraction. Philosophical methodology is also susceptible to implicit normativity. If the correctness of analytic-philosophical method is a shared assumption amongst the interlocutors a writer takes herself to be in discourse with, there is no practical need to distinguish descriptive and normative claims about that method. The positive normative claims would be dialectically innocent.[16] If we take it for granted that analytic philosophers are employing the right method, the only practically relevant task left for us is to correctly describe what that method is.[17] The principle of charity also pushes us to take normative considerations into account when our purpose is "merely" descriptive.

The commitment I undertake in this thesis is to do normative methodology. This should be apparent from the normative language used throughout the introduction and forward. I also believe, perhaps more controversially, that methodological theorizing ought to be normative. I can only offer *prima facie* justification for the claim. If asked, "Why do we practice methodology in analytic philosophy?" my answer would

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[15] As a starter, judging philosophical method according to moral virtues is not the task of a philosophical methodologist. We are studying philosophy as a theoretical discipline, not as a moral community, and must judge it as such. Chapter three elaborates.

[16] This is not to say that we cannot legitimately demand justification from the theorist who holds that actual method is correct. The requirement is not exclusively for *revisionary* normative projects. When properly challenged, for instance in the way experimental philosophers have challenged mainstream philosophy, justification of that method is called for. There are such things as dialectical defaults, however, but the default positions are not unchallengeable.

[17] Although it is unclear how wholeheartedly Williamson assumes this, it seems that *The Philosophy of Philosophy* works partly on this assumption (2007: 6).

be that not all moves in the game of analytic theorizing are good moves, and that we need methodology to put into principle which ones are. If method were not put into principle in this way, we would not have an argument against the villain who, in a lie, claims to be doing good philosophy when all he does is produce analyses in which concepts are analyzed only in terms of themselves.

The justification of the normative claims I make in this thesis is in principle independent of their descriptive adequacy. However, it is important that the method I defend cannot be charged with the accusation of being unrealizable. Say there are no examples of this method's employment in the history of philosophy. Because of this, it is necessary to use examples from actual first order philosophy to elucidate the methodological discussion. If the methodology I defend maps on to good argumentative patterns with respect to these examples, that will serve as indirect evidence for the correctness of the methodology.

1.4 The reflexive character of philosophical methodology and the scope of discussion

At least in philosophy, methodology is reflexive: the methodology we practice is itself a part of philosophy. This makes it its own object of study. By implication, it is impossible to fully justify this thesis' approach to methodology, prior to its own proceeding. The claims I defend are themselves intended to justify the approach I take in defending them.

There is one remaining preliminary. Although I sometimes write "philosophy" and "philosophers" without qualification, the scope of my discussion is limited to analytic-philosophical methodology, and conceptual analyses in particular. Scope restriction is more easily seen as a requirement on descriptive methodologies, since they should not falsely claim that the method they describe is in use for a broader range of philosophical theorizing than what it in fact is. Tunnel vision of the possibilities and actualities of philosophical practice must be avoided. To illustrate, Gutting (1998) has loosely stated the difference between *analytic* philosophy and *continental* philosophy as one between a thought tradition that aims at explicating and harmonizing its own pre-theoretic commitments, and one that aims at moving beyond those commitments by generating new perspectives or fundamental commitments. This is but one division with methodological import. There might be more. Such

divisions weaken the viability of a determinate, general theory of philosophical method. So, descriptive methodologies ought to have their scope properly restricted.

There is a corresponding requirement on normative methodologies. Different goals may be embodied in the thought traditions that collectively make up philosophy, and these goals might require the governance of different methodological norms. We should be sensitive to the difference this can make, even if our purpose is normative. It is possible that some of my claims allow for generalization—so as to include traditions not denoted by the term "analytic philosophy"—but I make no argument to this effect. It is possible that philosophy should be methodically uniform across the board; it is possible that it should not. Whichever happens to be the case, I am concerned with what method we should use as analytic philosophers, and have no argument for the further generalizability of the claims I make.

2. What Intuitions Are

This chapter argues that the proper demarcation of intuitions—what distinguishes intuition from straightforward belief and other propositional attitudes—[18]is their being in a specific dialectical role, in addition to having an unsettled strength of doxastic commitment.[19] The first characteristic, the dialectical role, consists in the proposition being one the agent (for whom it is intuitive) is not aware of how he should justify holding. It requires context for a propositional attitude to play this role. The relevant parameter for such a context is what the agent is aware of during the period of time he holds a proposition as intuitive. Specifically, it is what the agent is aware of that he himself would have counted as justification for holding the proposition, had he been aware of it. For instance, I have an intuition that a non-round figure cannot be a circle. How I should proceed to justify this is beyond my current comprehension, so it plays the sort of dialectical role I am claiming for intuitions in general. On the other hand, I non-intuitionally hold that a slight majority of newborns are boys. To justify this, I would proceed to find and make reference to the statistics that prove it. The other characteristic I define intuitions by, that they have an unsettled strength of doxastic commitment, means that there is no telling, on the grounds of knowing that someone has an intuition, how strongly the intuiter assents to the proposition in consideration.

Because intuitions are propositional attitudes that are defined by these two traits, many circumstances call for "intuitive" and its cognates because of the expressive utility they provide us with. Here are some peculiar situations: No arguments are forthcoming for the maker of a claim, although she cannot see why anyone would oppose it. It is too time- or space demanding to produce an argument or explanation, or viewed as pedantic to do so. Someone is entertaining a claim she would have put forward without qualification; but she has reason to deny it, so she holds the proposition only momentarily and with qualification.

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[18] I have not been able to get a grip on what a non-propositional intuition would be like, so I have no unqualified argument for why intuitions have to be propositional. However, if intuitions are to be even candidate justificatory, their contents must be available as premises for reasoning, and premises must be propositional.

[19] I use the expression "doxastic" to denote a belief-like property, as what figures in all kinds of cognitive assent.

We give reasons for claims and in turn give these claims as reasons, but intuitions are not given reasons. Hence, we can say that modifying a claim as intuitive—in an argumentative context—is putting forward a substitute for an argument, since the contrasting alternative to holding a proposition as intuitive, in that context, is giving it an argument. (In addition, by the second trait, such modification expresses an unsettled amount of assent to the proposition under judgment.)

Other theorists have defined intuitions by such (supposed) traits as their epistemic status, phenomenology, etiology, their having certain contents, and their temporal immediacy upon elicitation. In this chapter, I argue that these intuition accounts are wrong. I do so only as interpreting them to be stating necessary conditions on being an intuition. Most of the accounts are "on to something," and they are, of course, made by intelligent, hard-thinking philosophers, so it is a virtue if my account avoids implying that these associations are made completely in the dark. In section 2.1 I survey the competing accounts. In section 2.2 I elaborate, illustrate and defend my positive view on intuitions as defined by dialectical role. In section 2.3 I explain why intuitions are not evidence, and why treating them as epistemically basic is wrong.

2.1 A map and guide to the territory of intuition conceptions

I will draw lines of disagreement by posing questions about candidate demarcations.

2.1.1 Does belief that p imply intuition that p?

Williamson (2007: 215) quotes van Inwagen as saying that our intuitions are simply our beliefs, except in some cases where they are only belief tendencies, that "move us" toward accepting a proposition. Lewis is quoted as holding a similar view, and it appears that Williamson agrees with them (2007: 220). This seems like an expression—ignoring for the moment the inclination-to-believe qualification—of the view that we have an intuition that p if and only if we believe that p.[20] It does not matter if the claim is for identity rather than equivalence, since it will suffice to consider the weaker claim. In this and the next subsection I will consider the two directions of this bi-conditional; first whether belief that p implies intuition that p.

[20] I am not aware of any more definite advocates of this view, but nevertheless include it for the purpose of mapping out the analytical possibilities in play.

There are some propositions we would never report intuition of, which are perfectly good belief candidates. I can sincerely say that I believe that lambs' wool is one of the best fabrics with respect to warmth and comfort, but it would be awkward if I said that I intuit that proposition. The question is: Is it also untrue or incorrect in a deeper sense than being merely an awkward thing to say? The defendant of the position in consideration could invoke some sort of Grice-inspired pragmatics story in response to such an example, in which it is true that I intuit the proposition about lambswool, but where it is prohibited by social custom for me to say that I do so.

A good reason to think that there is more to it than social custom, is that "intuition" is *systematically* (if not universally) not used when either of the following two conditions obtain, whereas nothing is wrong with "belief" in these circumstances. First, we do not speak of intuiting a proposition in the context of an explicit *inference* to the candidate intuitable from other propositions we are doxastically committed to. If I infer that Socrates was mortal since he was a human and humans are mortal, or from the evidence we have of his death, I am not having an intuition that Socrates was mortal. Second, we never say that we have an intuition when the candidate proposition is *evidentially grounded* by observation or experience. The comfort and warmth of lambs' wool are properties I—and I am sure many others—have experienced. Or if we are watching a cat play in the yard: that might elicit a belief that the cat is playing in the yard, but we would not report it as an intuition. In the absence of a counter-argument, I conclude that we can believe that p without having an intuition that p.[21]

2.1.2 Does intuition that p imply belief that p?

So belief can come apart from intuition, but can intuition come apart from belief? An initial reason to hold that it cannot is to avoid positing "entities beyond necessity," which in this case translates to positing excessive doxastic attitude kinds (section 3.1.4 says more about Ockham's Razor). However, this cannot stand in the face of a specific example of an intuition that p with no belief that p. Bealer has an example to

[21] Note that, in these first two subsections, I am not denying that the class of intuitables is equivalent with the class of believables, which would just be the class of propositions. Sosa (2007) and Bealer (1998) e.g., have proposed that intuition, in the sense they are interested in, is restricted in terms of the content it is directed at, in which case these two classes would be non-equivalent, but that is another defining question for intuitions, considered below (section 2.1.7).

this effect, where the content of the intuition is the Naïve Comprehension principle (Bealer 1996: 6). The obsolete axiom says that for every property, there is a set that consists of all the objects with that property. At first consideration, nothing seems wrong with it, but it is inconsistent by Russell's paradox. So we do not believe it in the light of the paradox, although we might continue to find it intuitive.

Bealer's aim, however, is to vindicate the evidential status of intuition. This is problematic, since the case is one where intuition is wrong and belief is right. Hence, if we grant that it is a genuine case of intuition-belief disagreement, the epistemic prospect for appealing to intuition does not look good. To make things worse, there is no complementary counterexample in which there is an intuition that p and no belief that p, where intuition is right in holding p. If there are such examples, it is implausible that they would have intuition come out better than belief in terms of justification. Belief formation is rationally demanded to have all available evidence considered (Elster 1983: 16). Intuition would have to fly in the face of available evidence to come apart from a rationally held belief. But that is a crime, epistemically speaking. In any case where there is evidence or argument available for holding p, it is just wrong for the agent to not take it into account. But if she does, then there is *only* belief and not intuition that p (since the attitude would be inferentially or observationally grounded). It is unimaginable that one can have an intuition that p but no belief that p when there is no evidence or argument available for holding p.

This does not show that Bealer is wrong in claiming that intuitions can come apart from belief; but to the extent they do, we better believe and not intuit. In fact, Bealer's example is positively conclusive on the issue of whether intuition can come apart from belief. Even so, we should not exaggerate the extent of belief-intuition disagreement. In most circumstances we will believe what we intuit to be the case, if we intuit at all.

2.1.3 Are intuitions inclinations to believe?

To make sense of the belief-intuition discrepancy evinced by examples such as the one above, some authors have recruited the idea that intuitions are *inclinations* to believe rather than the beliefs one is thereby inclined to form (Earlenbaugh & Molyneux 2008 e.g. and van Inwagen, as we saw above). This idea is not vulnerable to an example of Bealer's kind, since an agent can be inclined to believe something

without believing it outright. If I unreflectively watch a stick submerged in water, I might be inclined to believe that it is bent. But in this case I know better, so it is unlikely that the inclination will succeed in generating a proper belief that the stick is bent. Sometimes, however, inclinations succeed in producing belief, and rightly so, as when I am inclined to believe that the subject in the Gettier case does not know what he believes to be the case. In the literature, our denial of KNOWLEDGE to the Gettier subject is held as a paradigm intuition. So we suppose that intuitions are inclinations to believe, since it is consistent with the above example and is coherent with a paradigm intuition in philosophy.

First and foremost, this is a good suggestion, and I do not claim that it is false. But it is not as beneficial to our understanding of what intuitions are, as on the account I favor, which also makes sense of the fact that we are inclined to believe what we hold intuitionally. On this account, being inclined to believe that p is a natural and typical *consequence* of having an intuition that p. But as long as we have an independent construal of intuitions as a kind of attitude toward propositions, there is no reason to *identify* them with the causal-functional role they stand in to other kinds of attitudes, even if such causal relations are widespread or universal. If possible, it is better to make sense of intuitions in their own right.

2.1.4 Are intuitions occurrent?

Some authors speak of intuitions as judgments (Ludwig 2010; Weinberg, Nichols and Stich 2001, Gopnik and Schwitzgebel 1998). This suggests that intuitions are occurrent. That is, it suggests that intuitions are more event-like than state-like; that they are not inattentively held over a period of time without the contents being apperceptively intuited. Some make this candidate intuition-defining characteristic an explicit commitment. Bealer writes, "The view I will defend is that intuition (this type of seeming) is a sui generis, irreducible, natural (i.e. non-Cambridge-like) propositional attitude that *occurs episodically*" (1998: 207, emphasis added). It is unusual that we find both rationalists and experimental philosophers, along with naturalists—that is, both proponents and opponents of the use of intuitions—agreeing on a defining characteristic of intuitions, but this is one instance.

I am not certain of the (rationalist) motivation Bealer has for claiming that intuitions are occurrent attitudes. We might suspect that it is a part of his more general strategy

to distinguish intuitions from beliefs (for which he also recruits the idea that intuitions are "seemings" while beliefs are not). If intuitions are natural, as in Bealer's own words, it seems strange that they overlap so much with belief—hence the need to distinguish them by natural characteristics, such as "are occurrent."[22] Another rationalist motivation for treating intuitions as occurrent stems from the idea that intuition works in ways highly or perfectly analogous to sensory experience, so if sense experience is occurrent, intuition is too. The trust in this analogy also shines through in talk of intuitions as "experiences" (Goldman and Pust 1998: 179; Chudnoff 2010).

On the other side of the dialectic, experimental philosophers need an operationalized conception of intuition in order to conduct experiments. Weinberg, Nichols and Stich write, "As we use the notion, an epistemic intuition is simply a spontaneous judgment about the epistemic properties of some specific case" (2001: 5). Clearly, something is a spontaneous judgment only if it is occurrent.

Notwithstanding the above *motivations*, I do not know what *arguments* there are for holding that intuitions are occurrent attitudes. But pointing to a lack of argument for a claim is not yet an argument against it. So we need a counterexample to defeat the claim. Consider my intuition that the subject in a properly construed Gettier case does not have knowledge. I have not worked with KNOWLEDGE-analysis and the Gettier case in detail, so I do not have a principled reason that I can use to explain why it is so that the subject in the Gettier case does not have knowledge. It remains a commitment of mine despite not being positively justified. Sometimes I think of the case, and the intuition becomes occurrent, but for the most part I do not think of it. But all the time I am not thinking of the case, it seems wrong to deny that I have the intuition. The correctness of the claim occurred to me the first time I entertained the case, i.e. when I acquired the intuition, but it is not something I have achieved to make an argument for since then (or *read* an argument, for that matter). Why should it escape being an intuition when I am not thinking about it? If this is a plausible and adequately described scenario, intuitions are not necessarily occurrent.

[22] In this context, the counter-characteristic of being occurrent is being dispositional, and belief is predominantly regarded as dispositional.

As with the inclination-to-believe demarcation, I believe my own account makes sense of why intuitions are thought to be occurrent. I take this to be a virtue of the account in spite of my opposition to the claim as one of necessity. It is plausible that intuitions are usually occurrent if it requires confrontation with a specific dialectical situation to have one's cognitive pro-attitude correctly ascribed as an intuition. Moreover, confrontations with dialectical situations usually *occur* for epistemic agents, and do not remain present. In the example of my intuition about the Gettier subject, however, the dialectical situation does remain present.

More importantly, we should not put much weight on this issue. Two factors go into this recommendation. First, the experimental philosopher's need for an essentially operational conception of intuition—what is expressed by immediate responses to probes—tends to make the concept inadequate for application to philosophers. When philosophers spend laborious hours thinking about cases and proper judgments about them, perhaps even changing their opinions, it is at best awkward to label their judgment "spontaneous." Second, the being-occurrent property of intuitions—whether essential or contingent to them—should not be a ground for pro-arguing the evidential status of intuition by pushing an analogy with sense experience. To the extent that one is experiencing something when one is thinking, this is not a part of the thinking, and it is not a reason for assigning evidential status.

2.1.5 Do intuitions have a phenomenology?

For some theorists, the idea that intuitions are occurrent is tied up to the idea that intuitions have a characteristic phenomenology.[23] Let us examine and criticize this idea in more detail. From the perspective of my own commitments, the claim that intuitions have a phenomenology is alien, so it is better to have a proponent state it. Here is Chudnoff:

[23] I will not treat the claim that intuitions have a phenomenology as the claim that they have a special feel to them. The reason is, first, that it is an impossible claim to assess unless it is specified in terms of feelings I, and other interlocutors, can identify. Second, to my knowledge, the most elaborate version of the claim—that intuitions have a phenomenology—does not seem to use the term in that sense (see Chudnoff below). There, "phenomenology" is used in a broader sense, as what pertains to intentional structure in general (i.e. more in agreement with phenomenology as conceived by Husserl, Heidegger etc.). For Chudnoff's conception, the structure is one between object and fact.

> Just as with the presentational phenomenology of perceptual experiences, the presentational phenomenology of intuition experiences is a correlation between two kinds of phenomenal property: an intuition experience possesses presentational phenomenology when in it you both seem to fact-*intuit* that p and seem to be *intellectually* item-aware of an item that makes it the case that p. (2010: 323)

There are two problems with this conception. The first concerns what it is to be "intellectually item-aware" of something. *Perceptual* item-awareness is not problematic. When I look around, I see objects. I see a computer, a bottle, a telephone, and many more things. These are items of my perceptual awareness. So what is it to be *intellectually* item-aware of something? Chudnoff provides a difficult-to-grasp example. The item in the example is a "many-many mapping from concave figures to convex figures that associates each concave figure with those convex figures that bound a greater area in a smaller perimeter" (2010: 324) (the proposition supposedly made true by this item is *every concave figure can be rounded out to a convex figure that bounds a greater area in a smaller perimeter*). In another example, the item of intellectual awareness is a complex arithmetical operation. Frankly, it is implausible that we are aware of these sorts of objects; never mind what object I am supposed to be aware of that makes it the case that the Gettier subject does not have knowledge. To make things worse, these items are suggested from someone who is out to defend the notion of intellectual item-awareness; they are not cherry-picked for my purposes of opposing such a notion. Object perception has no good analogy for thinking. Thinking is primarily performed at the level of thoughts, and thoughts are propositionally expressible. We can assume that there are abstract objects, but there is no eye of intelligence detecting these objects.

The second problematic feature of Chudnoff's account is the idea of an object making a proposition the case. For instance, consider my observation that there is a bottle on top of the table. What object makes this the case? The table does not make it the case that there is a bottle on top of it. But neither does the bottle make it the case that it is on top of the table. Perhaps *the presence* of the bottle makes it the case that there is a bottle on top of the table. In fact, we can grant this. But in no sense am I *item*-aware of the bottle's *presence*. I am item-aware of the bottle, not of its presence. Either we correctly specify an item we are aware of and wrongly hold it to make the relevant

proposition the case, or we specify an item we are not aware of that *does* make the relevant proposition the case.

The obscurity of Chudnoff's account makes it hard to assess. In the absence of other accounts of the phenomenology of intuitions, we can deny that intuitions have any specific phenomenology.

2.1.6 Are intuitions expressions of conceptual competence?

Kirk Ludwig is a proponent of the view that intuitions should be defined etiologically (2010: 431; 435). Specifically, on Ludwig's account, intuitions are distinguished by being explained by the subject's understanding of the relevant proposition (2010: 431).[24] As Ludwig says, intuitions "are to be conceived of as judgments or beliefs which are the product of our competence in the deployment of the concepts involved" (Ludwig 2010: 432). Ludwig does not hold this as an adequate conception of intuitions in general. It concerns specifically "rational intuitions," and these are distinguished from "physical intuitions," such as the intuition that a door will burst open if some specified amount of force is applied to it.

A reason to take this view is that it makes it intelligible to conceive of philosophy as an *a priori* inquiry based on intuitions (let us assume that it is), while not undermining the legitimacy of such a practice. If we are equipped with a conceptual competence and an ability to express such competence when we sit down to philosophize, then we both have a source to draw upon to articulate our theories, and the warrant to do so, granted that our possession of the concepts is in fact competence and not incompetence. If this is what we do, then we do not need something like observational data to justify the theories we construct. In that way, the idea of philosophy as an *a priori* inquiry is done justice.

A first problem with Ludwig's account is that it forces him to hold that intuitions are veridical (Ludwig 2010: 438). As products of *competence*, we must grant that they are

[24] He is not alone in holding this view. Sosa (2007: 101) is another prominent defender. Moreover, this is not the only way to define intuitions etiologically. A naturalist version is due to Michael Devitt, in which intuitions are "empirical theory-laden central-processor responses to phenomena, differing from other such responses only in being fairly immediate and unreflective, based on little if any conscious reasoning" (2006: 491).

correct, or at least generally correct. This is inconsistent with the psychological literature on intuitions. There we find an abundance of false intuitions (Shafir 1998 e.g.). So, either Ludwig's account is disproved by those results, or he must hold that the psychological literature is concerned with something else than he is. Although it does not prove the account to be false, incompatibility with the psychological conception of intuitions is unfavorable, and we deserve a reasonable explanation of the diversion (cf. section 3.1.2).

Secondly, while we need not be opposed to restricting a metaphilosophical discussion to specifically conceptual intuitions, there is no point in doing so at this stage if we can develop an independent and robust conception of what intuitions are, regardless of their being conceptual or empirical or whatnot. We can then restrict our discussion on the basis of such a conception if restriction is still needed. Developing a robust general conception of what intuitions are is in line with the norm that we ought to have our concepts integrated (section 3.1.4). There is no point in suffering the conceptual disintegration involved in having disparate conceptions of empirical and conceptual intuitions.

I claimed that the inclination-to-believe conception and the intuitions-are-occurrent demarcation are shown to be intelligible—as associations of what intuitions are, not necessary conditions—in the light of my own account of intuitions. The same is true of Ludwig's idea that intuitions are expressions of conceptual competence, although explaining why is more tricky. When we hold that intuitions are expressions of conceptual competence, we are in effect asserting a justificatory status for what is claimed intuitionally, while at the same time making that justification *opaque* to us. Competence comes with a justificatory status for its expressions (or else they would be expressions of *in*competence or *non*-competence), but exactly how they are justified does not show in calling them expressions of competence. It might be difficult or even impossible to articulate the justification intended by asserting that some performance is competently executed, but that would only be because an indefinite complex of factors determines that competence. In other words, how to flesh out why something is a product of competence, by replacing that denomination with the *reasons* for why doing so is correct, is not usually transparent. The case of conceptual competence illustrates this nicely. If a conceptually competent agent

encounters a pair of claims, p and ¬p, and applies the concept CONTRADICTION to that pair, it is not immediately forthcoming how we should justify the application. Restating the pair of claims and noting that it is *obvious* that the pair constitutes a contradiction is no reason. We could say, "that is how the concept CONTRADICTION is used," but that would only beg the question of whether that proclaimed use of CONTRADICTION is correct. This is not to say that it is impossible to justify such concept deployments, but it is certainly difficult.

My account of intuitions makes sense of why being an expression of conceptual competence is associated with being an intuition. The considerations in the previous paragraph make that clear: The justificatory opacity of expressions of competence is right in line with the lack of explicit argument and observable evidence I claim for intuitions in general. Intuitions that are products of conceptual competence may comprise a remarkable class of intuitions, but they are not the only intuitions. For instance, consider "Intuitively, the combined presence of the teenager's mother and friends caused him to try and behave in incompatible ways." This assertion is not an expression of a distinctively conceptual competence. Rather, it is an expression of experience from the first and third person of that kind of social circumstance. So we had better make sense of the relation between conceptual competence and intuitions without committing ourselves to the view that intuitions are *necessarily* derived from such competence.

2.1.7 Are intuitions only held toward certain contents?

The suggestion that intuitions only have certain kinds of content parallels the previous demarcation if we take *the conceptual* to be the sort of content intuitions take up. More typically, the claim is that intuitions have a *modal* content, i.e. necessities and possibilities (Bealer 1998, Sosa 2007: 101).[25] A clear-cut example that confirms this view is the intuition that zombies are possible. Another one is the intuition that 2 + 2 = 4, which is necessary.

However, consider the following case. Anne is confronted with a properly construed Gettier scenario. She intuits that the subject in the scenario does not have knowledge

[25] Bealer's claim is weaker than Sosa's. He claims only that *the most important* class of intuitions is modal (Bealer 1998: 213).

that p. Taken at face value, the content of Anne's intuition is neither a possibility nor a necessity. She simply intuits that the subject in the Gettier case does not have knowledge that p. Defenders of the view in question might want to argue that I am ascribing the wrong intuition to Anne. Her real intuition, the objection would go, has a more complex content. For instance, they could say that the content of Anne's intuition is that, necessarily, the subject in the Gettier case does not have knowledge that p, given the case description. But it does not suffice hold that Anne has this latter intuition. They must also have an argument for why we should deny that Anne has the former intuition. Moreover, they must have a gerrymandering strategy of this sort available for every intuition with a face value non-modal content. In the light of this, the restriction of intuition contents to modal propositions is unmotivated.

As for other restrictions on the sort of content intuitions can have, I have no argument. As far as I am aware, no other restriction has been proposed. The temptation to demarcate intuitions by their content should fall away when the dialectical role of intuitions is comprehended. On the account I promote, any sort of proposition can in principle be the content of an intuition. Intuitables are just believables, and these are just propositions. There might be some determinately specified class of propositions we are especially *prone* to hold as intuitions, but that would be because they are propositions we find it hard to produce reasons for holding (that are not question begging), or ones for which we lack an established method of proof. For claims about possibilities and necessities, for instance, we do not have an established method of proof.[26] This might be the reason why some intuition theorists have taken them to comprise the class of contents intuitions can be held towards.

2.2 Elaboration and defense of my intuition account

My positive proposal is that we should identify intuitions by two characteristics; an unsettled strength of assent and a dialectical role. First, then, intuitions are cognitive pro-attitudes. They count as assent to the intuited propositions. But the strength of assent they imply is unsettled. Straightforward assent is belief, but intuition does not imply straightforward assent. The assent to p involved in having an intuition that p is compatible with straightforward dissent to p, but is not itself a dissenting attitude

[26] I.e. I do not regard the conceivability criterion on possibilities to suffice as a method of their proof (Chalmers 2002). For discussion, see the collection of essays in Gendler and Hawthorne (2002).

towards p. The assent can be weak and disconnected from belief—so to imply only a slight inclination to believe, as with finding Naïve Comprehension intuitive, even in the light of Russell's paradox—but it can also be strong, as with my intuition that causality is a relation between events. So the attitude taken in having an intuition that p is an, at minimum, surface level pro-attitude that p.

Second, the proposal is that intuitions play a specific dialectical role. The role consists in the intuited proposition being one the intuiter is not aware of how she can justify holding. Argument and evidence that would—according to the agent—justify holding the proposition is not present to her. Importantly, the kind of context that allows for a proposition to play the relevant role is terminable. What we are aware of changes over time. We learn, discover, think harder about things, and so forth. Because of changes such as these, propositions can discontinue playing the role of being justificatorily unsupported by what an individual is aware of. Hence, an individual can achieve justification of an intuition, but in that case the attitude ceases to be an intuition. An intuition can turn into a well-argued belief, but there is no general recipe for when or how that happens. Conversely, an intuition can lose its grip on us, for instance when we familiarize us sufficiently with the arguments and evidence against it.

Since what makes a cognitive pro-attitude an intuition is only a *present* lack of satisfactory reasons for holding the intuited proposition, it is not so that it is impossible to justify holding those propositions. There can be good, external reasons for a subject to hold a proposition—ones that would have had the subject's approval—but in the context of intuiting it, the subject is not aware of those reasons. These contexts can be limited to a couple of pages in a book, where an argument has yet to be stated, or they can last indefinitely, say, if the particular commitment is so fundamental to us that we would have to step outside ourselves, so to speak, to justify it. That is why the role we assign to intuitions is dialectical and not epistemological in a deeper sense. If the contents of intuitions were unjustifiable in principle, but nevertheless assented to as intuiteds, they would have a very special epistemic role. In that case we might even think they have a foundational role, in analogy with perception. But since the contexts in which intuitions are unjustified are not eternally present, we can resist such a conclusion. We do not hit the proverbial rock bottom the

moment we find some proposition intuitive, even if it is inferentially basic for the time being.

In philosophical discourse, putting forward a proposition as an intuition is expressing the proposition with what functions as a substitute for an argument. Its being the content of an intuition substitutes its being the conclusion of an argument. Epistemically, it is a poor substitute, since it does not make justification for the claim transparent. However, it serves other purposes, such as initiating an argument where, had there been no claim playing that role, a regress of arguments would obtain. This can happen if it is unclear what would count as a non-controversial starting point, so that some practical concession must be made to that fact, viz. that an expression of an intuition must be accepted as a starting point for discussion. In such circumstances, saying "intuitively, p," when initiating an argument, as opposed to claiming the premise outright, is just being honest about the stringent demands of justification in play and the difficulty of meeting those demands.

As reason demanding creatures, we would like to give reasons for all the commitments we undertake by way of assenting to propositions. Seeing as this is practically impossible, it is good that we can put forward propositions in discourse without expressing full-blown, definite assent. In that way we can keep our reason-demanding integrity intact, while holding open the *possibility* of justifying the tentative commitments thus expressed. A contrasting approach is to view this as a murky strategy for flouting argumentative responsibility. But we cannot know how we should argue for a claim unless we know what the relevant challenges to it are.

In defending my conception, I rely on some desiderata. One has already been presumed, which is that the account makes sense of why some traits have been associated with intuitions, by leading theorists working with the issue. However, this could hardly convince anyone to take my view. The main points in its favor are its reasonable degree of satisfaction of the norms I take to constrain conceptual analysis (cf. chapter 3), and its making sense of explicit intuition appeals in first order philosophical discourse, as well as in non-philosophical discourse. Sections 2.2.1 and 2.2.3 show how the account makes sense of various intuition appeals. The following paragraph shows how it satisfies the norms for conceptual analysis.

First, the account makes the concept of an intuition intelligible to us. That is, the account advances our understanding of what intuitions are. We have a better grip with what it is to not know how we should justify a claim, and what it is to tentatively accept a proposition, than the pre-theoretical grip we have on what intuitions are. This marks a contrast with Chudnoff's phenomenological conception, for instance, on which we were left puzzled with what sorts of items that present themselves to us intellectually, and how they make the intuited propositions true. As another example, saying something like, "If a subject S has an intuition that p, it *just seems* to S that p," does not count as satisfying this norm. Second, the account avoids disintegrating our concepts so that we must understand what intellectual, philosophical intuitions are by themselves, apart from understanding what everyday intuitions are, or what specifically physical intuitions are. We saw the conceptual-competence-account of intuitions fault in this regard. Third, it provides expressive utility that we are not granted by considering intuitions to be just inclinations to believe. In addition to expressing openness-of-assent, intuition appeals express, on my account, the dialectical role an intuition plays in a context. It is useful to be able to express this role, and it is a positive feature of the account that it explicates that role. Fourth, the account serves explanatory purposes by distinguishing the proposition's occupation of a dialectical role from its epistemic role. On premises of the account, an intuition can be dialectically foundational without being epistemically foundational. Section 2.3 discusses the epistemological issue in detail.

2.2.1 Intuition appeal in first order philosophical discourse[27]

If you decide to go on a hunt for occurrences of "intuition," "intuitive" and "intuitively" in the analytic-philosophical corpus, do not expect to spend an hour or two and come back happy, equipped with an abundance of examples. *Explicit intuition appeal*, while not outlandishly rare, is not as frequent as is suggested by the idea that the only thing we have to go on in philosophy is our intuition. We could also be more liberal about what we are counting as an intuition appeal. There are, after all, many expressions that have at least a similar effect to the explicit intuition appeal locutions. Here are some examples: "It seems that p," "Obviously, p," "Pre-

[27] I am in debt to Cappelen (2012) for the idea that we should review actual intuition appeals in philosophy.

theoretically, p," "Naturally, p," "It is plausible that p," "Clearly, p," and maybe also "It is a platitude that p" and "Common sense tells us that p." Even if it is fine to treat these manners of speech as intuition appeal-implying or intuition appeal-indicating locutions, as we encounter them in philosophy, I do not have the space to warrant that treatment. Therefore, the examples I include here are explicit intuition appeals only.

Moving on to the examples, we aim to confirm that the use of "intuitive" and "intuitively" permits viewing them as expressing attitudes that are defined by not having a definite strength of assent; and that these tentative commitments occupy the dialectical role of being unargued for and ungrounded by observational evidence. I have put occurrences of "intuitive" and "intuitively" in bold type to ease the reading.

Here is Russ Shafer-Landau—a moral realist, i.e. non-naturalist—admitting a premise for his discussion:

> If a division between the natural and non-natural cannot be maintained, then much of the impetus for the metaphysical critiques of ethical non-naturalism lapses. But let us suppose, as seems **intuitively** right, that some such division can be vindicated. (Shafer-Landau 2003: 80)

The intuition in this passage is that some division of the natural and non-natural can be vindicated. Shafer-Landau is not convinced that the division can be maintained, and it would only cause his theory less suspicion if it could not. His lack of conviction can be read out of the first, conditional sentence, along with the lack of an outright denial of its antecedent. Rather, the next sentence invites us to *suppose* its denial; i.e. suppose that the division can be maintained. Treating intuition as open-ended in the strength of assent it involves is consistent with this, and it makes sense of Shafer-Landau's tentative acceptance of the target proposition. Moreover, no reason is given to accept the possibility of vindicating the division between the natural and non-natural, and neither are we in the vicinity of any observational evidence proving such a possibility. Hence, the intuition appeal also plays the right dialectical role.

This intuition appeal is typical for philosophers in that it involves a claim that is, as far as I am concerned, very difficult to give a non-question begging argument, so accepting it as a matter of intuition works as a practical concession to that fact. Perhaps it is special since it involves a claim (the truth of which) the author does not

care much for. He would do fine without a division between the natural and non-natural, but he is admitting it as a premise for his discussion nonetheless.

James Woodward makes explicit appeals to intuition frequently in his book on causal explanation. Here is one occurrence, which initiates a paragraph in a section on the devices he employs for representing causal relationships:

> **Intuitively**, variables are properties or magnitudes that, as the name implies, are capable of taking more than one value. Values (being red, having a mass of 10 kilograms) stand to variables (color, mass) in the relationship of determinates to determinables." (Woodward 2003: 39).

Here, little doubt is indicated on behalf of Woodward's assent to the intuited proposition, namely, that variables are properties or magnitudes that are capable of taking more than one value. So it is consistent with treating intuitions as involving unsettled strength of assent. When we compare with the Shafer-Landau passage, we now see why an indefinite *range* of assent-strengths is required of a conception of what being an intuition is.

The proposition put forward in the passage also plays the dialectical role we would expect from my account. Woodward's purpose is to analyze causal relationships by treating the causal relata of candidate relationships as two-valued or many-valued variables. Since variables play such an important role in his analysis, he explains what they are, presumably in the errand of making his theoretical foundation transparent to the reader. For this specific commitment of his theory—the use of variables and the explanation of what they are—the transparency stretches no further. The proposition *initiates* a quick account of what variables are, and its own lack of argument is admitted in the moment of modifying the claim to it as a matter of intuition. The intuition has—similar to Shafer-Landau's intuition—the characteristic that it is difficult to give positive reasons (however strong our acceptance of it is).

Tyler Burge appeals to intuition in this passage, when defining his ontological terms:

> I use "*property*" and "*characteristic*" interchangeably. Properties are shareable aspects of particulars, or—at a higher level—of properties, relations, or kinds. Properties are aspects of single entities. *Relations* hold or fail to hold between, or among, more than one entity. Thus, **intuitively**, sphericality is a property; and being-bigger-than is a relation.

> The distinction depends on one's level of analytical or ontological rigor. Tallness is **intuitively** a property, but, on closer reflection, tallness is seen to be relative to a comparison class. Speaking **intuitively**, tallness is a property of Shaquille O'Neal. But, speaking more analytically, Shaquille O'Neal is tall for a human being but not tall for an upright physical body on earth. I sometimes use "property" to cover what are, at some levels of analysis, relations as well as properties. (Burge 2010: 55).

Four different propositions are held as intuitions here. They all articulate examples of the ontological categories Burge is defining. The attitude expressed towards the first two—that sphericality is a property, and that being-bigger-than is a relation—is in line with the suggested consequence of my account, that the assent taken to them has a positive but unsettled strength. Playing the illustrative role they do, they are not the main point of Burge's discussion, and they could easily be replaced with other examples. Burge does not need to express unqualified assent to them: He would not want the well-foundedness of his categories to hinge on badly chosen examples. My account explains the use of "intuitively" for that purpose. The second pair of propositions—that tallness is a property, that it is a property of Shaquille O'Neal—are also put forward with positive but unsettled assent. Burge's use of "but" makes his unwillingness to full-blown assent explicit for these two intuitions, as he presents us with a consideration that could defeat the claim that tallness is a property. We do not have to agree with Burge that relativity to a comparison class makes a candidate property into a relation (when judged at the level of analysis where such relativity is unveiled), but his assertion that it does, suggests that it might be correct to interpret him as holding overall *dissent* to the claim that tallness is a property (of Shaquille O'Neal or whomever). This interpretation of Burge is reasonable if he accords higher authority to the more rigorous levels of analysis than those associated with intuition. But even if we grant this, it remains false to say that he expresses *no* assenting attitude to the proposition that tallness is a property, or else his claims about the levels of analysis and their corresponding judgments would lose their meaning. The assenting attitude he takes is that of intuition.

Furthermore, no reasons are given for holding any of the intuited propositions. No reason is given to think that sphericality is a property, that being-bigger-than is a relation, that tallness is a property, nor that tallness is a property of Shaquille O'Neal. Despite being without argument in the text, and therefore counting as intuitions in that

dialectical circumstance, these commitments could be expressed in other circumstances where they would not count as intuitions. As a contrast with Shafer-Landau's intuition and Woodward's intuition, it does not seem equally difficult to produce solid arguments for these commitments. In that event, they cease to be intuitions. Here is one argument, for sphericaliy's status as a property: properties are aspects of single entities; sphericality is an enclosing geometrical form; being enclosed by a geometrical form is sufficient to be a single entity, and the form is thereby an aspect of that entity; therefore, sphericality is a property. To argue that tallness is a property, we might reason thus: properties are aspects of single entities; if *x* is vertically measurable, and there is a comparison class that *x* sufficiently outmeasures, *x* has tallness as an aspect of it; Shaquille O'Neal is vertically measurable (2,16 m) and there is a comparison class he sufficiently outmeasures (the human population); Shaquille O'Neal is a single entity; therefore, tallness is a property. The arguments show that it is not impossible to pro-argue intuited propositions in ways that are not wholly question-begging. Expressing them as intuitions might be a work-saving device. This does not take away from their status as intuitions in the dialectical situation of p. 55 in Burge's book.

Finally, John Perry recapitulates an "intuitive argument" in the following passage:

> Let's review. In the first chapter, I introduced an **intuitive** argument, which I called the "experience gap argument." There seems to be a huge gap between the awareness we have of our own mental states, which we might express by identifying them as "this sensation" or "this feeling," and the sort of knowledge we could have of a brain state, using any ordinary or scientific observational techniques we can imagine, from tiny people in tiny boats inside of brains, to autocerebroscopes, to the actual techniques of brain scientists. The zombie argument, as I interpret it, is one attempt to turn this **intuitive** problem into a solid argument against physicalism… (Perry 2001: 93).

Taken at face value, the intuitional content in this passage is an *argument* and a *problem*. Since "this intuitive problem" functions as an anaphor in the last sentence, Perry really only has one intuition in this passage, described alternately as an argument and a problem. This presents a challenge to my account of intuitions: I have stated that only propositional intuitions are of interest, whereas the intuition appeal made here *is* of interest but the content is held to be an argument, which we can understand as an inference. First, we should see that the content attached to the

intuition appeal is what is expressed by the sentence beginning with "There seems to be a huge gap…" and ending with "…the actual techniques of brain scientists." Thus, the content of the intuition is that *there is a huge gap between the awareness we have of our own mental states… and the sort of knowledge we could have of a brain state*. This, however, is no argument. Perry might be drawing on a usage of "argument" on which reasons/premises = arguments, even when they are stated without a conclusion. But my account does not preclude reasons or premises as intuitions. The only problem for my account would be if the *conclusion* of an argument were treated as an intuition, since that goes against the requirement that it must not be explicitly reasoned for in the context in which it is an intuition. But there is no reading of the above paragraph on which the intuited proposition is a conclusion. Instead, it is an unargued claim.

That Perry's assent to the content of the intuition is positive but unsettled in strength can be verified. The position he defends in *Knowledge, Possibility, and Consciousness* is physicalism. However, he allows for one sort of duality on which the noted "huge gap" can be explained as being between ways of knowing and not between the things or states that knowledge is about. Therefore, on his view, it is just fine to say that there seems to be a huge gap between the awareness of our own mental states and the knowledge we can have about brain states. Hence, I conclude that Perry's intuition appeal is made sense of by both the defining traits I attribute to intuitions: Positive assent, but unsettled in strength, and the dialectical role of being unargued for and ungrounded by available evidence.

2.2.2 Taking stock

Intuition appeals are not easily collected as data, and even if they were, providing well-argued interpretations of a representative sample is not possible within the limits of any kind of philosophical paper. Thus, I cannot say with perfect certainty that there are no intuition appeals in first order philosophy that cannot be interpreted with the conception of intuitions I promote.[28] If there are any, I am committed to hold that they are improperly appealed to as intuitions; that the author is misinterpreting his own attitude as an intuition, when it in fact is not. But my conception is rather weak. I do

[28] However, I do address a concern that my conception of intuitions is unfit to account for moral intuitions, since they are thought to simultaneously justify theories and be justified by them. See below, section 2.4.

not place high demands on intuitions, for instance by claiming that they must be rational, or that they must be derived from a special competence, or even that they must be occurrent. This makes it less likely that the account will be contradicted by the intuition appeals I have not had the time and space to survey. Although weak, the conception is sufficiently informative. It suffices to assess the epistemic status of intuitions and it has already sufficed to make sense of the philosophical intuition appeals we have surveyed. In the next section (2.2.3) we will see how the account also makes sense of non-philosophical intuitions. In section 2.3 I assess the epistemic status of intuitions.

2.2.3 Making sense of non-philosophical intuitions[29]

Philosophers are not the only people with intuitions. We should not disconnect our practice of appealing to intuitions from the ordinary practice of doing so. On some intuition conceptions, we cannot apply "intuition" to anyone but the most intellectual individuals in the act of pondering their conceptually competent judgment about hypothetical scenarios. The way to do this is to place too high demands on the conditions of intuiting. The problem with this is, first, that questions about the epistemic worth of intuitions tend to become trivial, and our real problem becomes identifying the cognitive conditions wherein such intuitions are attained and maintained. The other problem is the conceptual disintegration such high demands involve from the everyday concept of an intuition. Imagine if something similar were done to the concept of a belief, so that the beliefs philosophers were taken to have, were of completely different nature than the beliefs of non-philosophers. To not err in this regard, I shortly discuss a couple of usages of "intuitive" outside philosophy, and how my account makes sense of them.

One use of "intuitive" is directed at how some participant of a skill-intensive practice goes about performing that practice. The playing of games and sports—such as chess, card games, soccer, tennis and so forth—are often assigned the label "intuitive." These intuitive performances are especially remarkable when they belong to the top contenders of the respective disciplines. One might have expected that their moves were carefully calculated, but the intuitive execution of those moves goes against that

[29] My realization that a philosophical account of intuition ought to make sense of—rather than distinguish itself from—non-philosophical intuition is also in debt to Cappelen (2012).

idea. My account makes sense of these attributions of intuitiveness. The top contenders have so to speak internalized the correct moves. They do not have to reason for them in the heat of the performance. Instead, their choice of the right moves has to do with some kind of immediate recognition upon consideration. This means that the contents of their intuitions—the propositions that express the candidate moves—are not given explicit argument in the context of intuiting them, and such argument is not within easy reach for the practitioners. In further agreement with my account, this does not mean that they cannot be given reasons. For instance, non-practicing experts might be able to produce those reasons. Moreover, we should not forget the intuitive performances of sub-optimally skilled practitioners. Focusing only on the intuitions of top-notch practitioners leads us to believe that the contents of intuitions have some guaranteed correctness pertaining to them in virtue of being the content of an intuition. But they do not. If anything, an intuition is likely to be correct if a well-trained practitioner holds it, but that speaks to the quality of the training and not to the resultant proposition's being the content of an intuition.

Physical and digital appliances are also ascribed intuitiveness. A remote control, for instance, can be intuitive, and so can the interface of an operating system, or a state-of-the-art kitchen tool. The use of "intuitive" here reflects the ease we have with using the appliance. For instance, if a remote control is intuitive, we do not have to spend time searching around to find the standby-button. In part, these ascriptions of intuitiveness derive from hard-to-pinpoint reasons for the practical suitability of the appliances they are about. It also derives from our rich experience with appliances such as remote controls, operating systems and kitchen tools. Such experience gives rise to a sense of how these appliances should be designed; to some extent independent of their in-principle practical suitability (arbitrary custom can diverge from practical suitability). The account of intuitions I propose makes sense of these ascriptions of intuitiveness. Our reasons for claiming that the way an appliance is configured is good are not necessarily forthcoming for us when asked why. There are many factors determining such goodness of configuration. Instead, the propositions that express how we think the appliances should be configured are held as intuitions of ours—right in line with the conception I promote—and they are what underpin the intuitiveness of the appliances they are about.

2.3 The alleged evidential status of intuitions

We are now in a position to assess the evidential status of intuitions. Some authors claim enjoy such a status (Bealer and Bonjour e.g.), but far more common than being an explicit claim, it has been treated as a presupposition of philosophical practice. The oversimplified picture is this. A philosopher comes up with an analysis of a concept that articulates individually necessary and jointly sufficient conditions for the truthful application of the concept. The author can provide some cases to illustrate the truth-preserving goodness of the analysis, or we might think of these cases ourselves. Either way, the analysis's justification derives from its agreement with what we would say about the cases. In other words, its justification derives from its agreement with our intuitions. Then some other philosopher might come up with a counterexample that defeats the analysis: either one for which our intuition tells us that the conditions obtain but the concept should not be applied, or one in which the concept should be applied but the conditions do not obtain. Then the original analysis is either defended (by showing how the counterexamples do not work), or revised (so that it becomes immune to the style of counterexample put up against it), or discarded. Lather, rinse, repeat. But what kind of serious theoretical enterprise grounds its evidence on intuitive judgments? This challenges analytic philosophy's very right to exist. We must therefore assess the epistemic status of intuitions.

I think that once we properly disambiguate the question of whether intuitions are evidence, its answers are fairly obvious. To do so, we must distinguish the content of an intuition from the having of an intuition. That is, we must ask whether *intuitings* are evidence (for their contents), and we must ask whether *intuiteds* are evidence (for other propositions). To the first question I shall argue that the answer is no. To the second question I shall argue that the answer is, they can be, but that is only by being potentially true propositions we could use in inferences, and a proposition's being intuited is irrelevant to that status. After arguing for these answers, I will comment the idea that intuition is a *basic source* of evidence.

Despite my negative answers to these questions, I am not out to eliminate intuition appeals from philosophical discourse. But our understanding of their significance needs revision. They have a practical and communicative significance, not some inevitable and ultimate epistemological significance. A consequence of my view is

that some ways of treating intuitions are bad. Specifically, it is wrong to treat intuitions as *data*, even if we add that we must assign different degrees of importance to our various intuitions. Intuitions are not to philosophical theory what experimental and observational data are to empirical science. Treating intuitions this way overlooks the possibility of pushing our arguments deeper than the current level of intuitions; it alienates us as the holders of intuitions, as if we could not revise our commitments; it occludes our opportunity to justify philosophical theories in other ways, specifically by the norms I articulate and defend.[30]

2.3.1 Having an intuition that p is not evidence that p

The obtainment of a cognitive state is not in general evidence for the content of that state. Many believe that an omnibenevolent, omnipotent and omniscient being exists, and many believe the contrary, but the fact that people believe these propositions is not evidence for their truth. If having a belief were determinate evidence for its content, we would have to deny that both parties to the above disagreement believe what they presumably do believe—since it would lead to inconsistency—but that denial is false and unfruitful.

Consider a far-fetched possibility. Perhaps there is some tidy way of individuating beliefs or intuitions, some way of tracking down their occurrence, and some way of ascertaining their truth values, so that we can make a count of the true ones and the false ones. We might just find that most are true. Could we not then make a statistical argument for the havings of beliefs and intuitions, that having an belief or intuition that p is *probative* evidence for p? Such an argument is in spirit with epistemological externalism, popular as that is: In this case we would find that cognitive states are reliable indicators of the truth of their content.[31] The main problem with this suggestion is not so much that it is impossible to make a sound probability conjecture for intuitions, or that the probability for truth is too low to assign reliability. The problem is rather that it is just useless as a tool for finding out what is true. First, to

[30] Albeit, in satisfying the norm to conserve content (section 3.2) or in showing how some analysis dissatisfies it, it is quite natural to appeal to intuition, even if those appeals cannot constitute strict evidence for the analysis' satisfaction/dissatisfaction of that norm.

[31] Much of the debate over the evidential status of intuitions has been framed in terms of their relative reliability, so thought experimenting about truth-statistics is relevant to the debate.

provide the statistical data, we must already know which beliefs and intuitions are true and which are false, but with this ability we should not need to resort to external, statistical measures to judge the truth or falsity of new belief and intuition candidates. Second, on the basis of the probability conjecture, there is no further telling which intuitions are true, and which ones are false. Unless other truth-implying aspects are factorized, it can only remain a conjecture. But if they are, then those are the truth determiners for the increased probability from 0.*x* to 1. While it is simply implausible that we will find an external determiner of truth, we can be certain that *being held as intuitive* is not it.

This amounts to an argument that we would be left in the dark if all we had epistemically at hand were knowledge of a range of intuitions and their generic probability of truth. If we are to make anything of the claim that having an intuition with a particular content is evidence for that content, it must be as a claim with methodological import. But philosophy is not the place to found an argument on a probability conjecture. Even if some statistical significance could be assigned to an intuition's likelihood of truth, it would be methodically wrong to invoke such statistics. Surely, if the experimental movement in philosophy has shown anything, it is that people can disagree about what they find intuitive. If our method were based on the externally conceived truth probabilities of intuitions, we would not get off the spot when having an intuition disagreement.

No one would argue that having a belief is (probative) evidence for the content of that belief. But as I showed above (section 2.1.2), intuitions are no better off than beliefs. So we should not treat the having of an intuition as evidence for its content. What makes intuitions special is their occupation of a dialectical role. No positive epistemic worth should in general be accorded to that role. There is no tie between being true and being the content of an intuition.

2.3.2 The contents of intuitions can justify, but their being intuited is irrelevant to that status

Any proposition can be the content of an intuition if the circumstance is right. The class of intuitables is coextensive with the class of believables. If what would justify holding a proposition is not apparent to an individual considering it, but he nonetheless finds himself assenting to it, then the proposition is intuited in that

circumstance. Thus, any non-intuited content of a belief can *become* intuited even if it has not, as of yet, been so. This can happen, say, if an individual holding a belief is allocated to a context in which what the individual previously held as good evidence for the belief is defeated or challenged.[32]

Unless recourse is made to the previous question—whether the having of an intuition is evidence for its content—nothing about a proposition's being intuited is relevant to its evidential status. For some proposition p to be evidence for another proposition q, p must be true and it must be possible to infer q from p. Since having an intuition is not evidence for its content, that content's actually being true has nothing to do with the fact that it is the content of an intuition. Whether or not some individual finds it intuitive, and whether or not the majority of people find it intuitive, the proposition that *variables are capable of taking more than one value* is true (cf. Woodward above). Hence, it can be evidence for other propositions, such as the proposition that *if we can only assign two values to something, say, an event's occurrence or non-occurrence, that does not preclude treating it as a variable*. Since any proposition can be evidence for some other proposition, and any proposition can be intuited, given the right person in the right circumstance, then trivially, (some) intuitions are evidence. That a piece of evidence happens to be the content of an intuition makes no difference to its evidential status.

Interestingly, this also shows that it is futile to lament the use of an intuited proposition as a premise in an argument just for being that: an intuition. To assess the correctness of using an intuited proposition for justifying another claim, we must give reasons for and against holding the proposition itself; we cannot remain satisfied with noting that it is the content of some (relative to truth) arbitrary attitude. Just as it does not matter for its truth whether some person finds a proposition *disgusting*, it does not matter whether someone finds a proposition intuitive. Thus, if an author puts forward a proposition affirmatively, but leaves it unsupported by argument—that is, she expresses an intuition she has—we cannot deny her entitlement to asserting that

[32] Cf. Williamson's example of someone who believes that there are mountains in Switzerland, but is confronted with the radical metaphysical theory that macroscopical objects do not exist (Williamson 2007: 223).

proposition (which would typically be a premise for her argument), unless we can give *content concerning* reasons to reject holding it.

2.3.3 Intuition is not a basic source of evidence

What has made it particularly tricky to discuss the evidential status of intuitions in philosophy is the widespread idea that intuition is a *basic* source of evidence (e.g. Bealer 1998: 218; Goldman 2007: 5).[33] This idea forms a part of the more encompassing idea that intuiting is analogous with perceiving. Let us assume, in the absence of better alternatives, that the kind of basicness in question is an inferential basicness; that intuitions are not inferred from other intuitions or beliefs, just as perceptual beliefs are not inferred from other beliefs. The supposition of this trait as not merely contextually valid has made it difficult for opponents of intuitions to argue against their epistemic worth. On the one hand, their criticism has been lamented as itself being based on intuition; that they are committing some kind of practical contradiction when arguing against the worth of intuition (Bealer 1992). On the other hand, the opponents of intuition do not want to blame perception for providing us with inferentially basic beliefs, so it becomes incoherent to blame intuition for exhibiting the same property. In response to the latter of these difficulties, Jonathan Weinberg has worked out a way to "challenge intuitions empirically without risking skepticism" (Weinberg 2007). He sophisticates the challenge to intuitions by shifting attention to some other traits of perception. These include the ability to detect unreliability when the conditions for perceiving are bad (e.g. in trying to see something through fog), having the deliverances of the sense organs checked against each other, and having robust theories about how the perceptual faculties work. The claim is that perception can mitigate its own fallibility on these grounds, and that intuition cannot.

While intuitions are in fact lacking in the respects Weinberg discusses, it is wrong that they cannot have their fallibility mitigated. Intuitions can change. They can be revised and honed through training and learning. In philosophy, with its demand to push arguments as deep as possible, it is difficult to see that intuitions are not just the

[33] Even if he supports the idea that intuitions are basic evidence, Goldman has rather low expectations to what intuitions can show. He thinks they elucidate our concepts, but, as a naturalist, he construes concepts as personal and merely psychological (Goldman 2007: 16).

ultimate rock bottoms. But, as shown above, it is not in principle impossible to justify holding the content of an intuition. If such justification for holding an intuited proposition is achieved, the proposition ceases to be an intuition in the newly sought context. However basic some doxastic commitments are treated in the dialectical circumstance we find ourselves in, we maintain the ability to plunge deeper in the inferential chains that would justify those commitments. What this boils down to is that intuitions are dialectically basic without being epistemically basic. Because they are not epistemically basic, we do not have to treat them in the caricatured manner I sketched above, as data our theories must satisfy. Instead, we must confront the content of each intuition in its own right. We must do this in the ordinary way of treating claims, by giving reasons for and against them; by challenging and defending them.

2.4 Objection

Some might object that my account of intuitions fails significantly to appreciate their role in moral philosophy. The objection can be motivated from the vantage point of accepting the model of Reflective Equilibrium for the justification of moral claims: General theories of moral right stand in a mutual justificatory relationship with particular judgments about what it is morally right to do, i.e. intuitions about the moral right. (In the process of achieving reflective equilibrium, neither part of this relationship is designated higher authority.) The direction of justification that is problematic for my account is where general theories justify particular intuitions; that much is excluded on the grounds of my account of the dialectical role of intuitions.

For the sake of argument, accept Reflective Equilibrium as an adequate model of moral justification. However, it is not impossible for my account of intuitions to accommodate the mutual justificatory relationship this model implies. All I must claim is that what functions as an intuition in one context (in the context of justifying a theory), does not function as an intuition in another context (in the context of a theory justifying the particular belief). That is, the solution is to associate the directions of justification within the Reflective Equilibrium model with different contexts, and accordingly claim that the particular beliefs are not intuitions in the context where they are justified by the general theory. Distinguishing contexts in this manner is not viciously artificial. When particular moral beliefs are put forward as

intuitions in the context of justifying a theory of moral right, it is crucial that they are themselves not taken to be justified by that very theory, in that very context. That would only make for a circular argument. However, in other circumstances, these same intuitables may very well be given reasons. If those reasonings are pushed deep enough, they may lead to invoking a moral theory. My claim is that in such a circumstance, the intuitable (but now justified) claim is not an intuition.

Here is a story to illustrate. A child goes into his neighbor's backyard unnoticed, and returns to his room with a toy belonging to the next-door boy. Upon discovering the mischief, his mother says to him, "Taking that toy was wrong!" The child, however, is being a nuisance and challenges her authority. He asks, "Why?" Since she is a fair parent, she explains why it is wrong, which is to say that she provides justification for her claim. She says, "Stealing other people's property is wrong, and that is just what you have done."[34] In this context, a theoretical consequence of my intuition account is that the mother's response is not the expression of an intuition. Rather, it is the expression of thoughtful judgment. Furthermore, we ought to endorse this conclusion, both on independent grounds—since discrediting her first judgment, by calling it an intuition, is wrong in the light of her articulate justification—and on the grounds that, if this should be regarded as "paying a price" to uphold a theory, it is a small price to pay in comparison with the gains the theory provides us with, by making intuition appeals intelligible and assessable.

[34] While this principle is not as general as the grand theories of moral philosophy, such as utilitarianism and Kantian deontology, it suffices to illustrate the point about intuitions.

3. Norms for Conceptual Analysis

The previous chapter has left us with a method up for grabs. It concluded that we cannot attribute epistemological significance to intuition appeals, whether that significance is positive or negative in value. In the context of philosophical inquiry, it is futile to blame or praise intuitions in general. Rather, the contents of the intuitions we encounter in philosophical discourse must be assessed one by one, in the usual way of treating doxastic commitments. It does not matter whether the attitudinal medium for a commitment is that of intuition. The content is not thereby shut off from the space of reasons, even if the intuition holder cannot or will not plunge in deeper argument in the context of her inquiry.

Williamson argues for a similar conclusion in Chapter 7 of his *The Philosophy of Philosophy* (2007), in arguing against so-called "judgment skepticism." A judgment skeptic is someone who denies the legitimacy of simply applying concepts in judgment, usually with respect for some limited domain of judgables. In Williamson's narrative, the typical judgment skeptic is a scientifically or metaphysically minded philosopher, so a good example is judgment skepticism towards folk psychological judgments. Williamson shows that there is no preventing that the arguments for such positions can be given whatever domain specific content, so that the skepticism would reach far beyond its intended victims. Unless we want to dismiss all judgments for being unascertainable, the formation of judgments cannot be the locus of doubt. It is unjustified to accord judgments in general a positive or negative epistemic value.

By denying epistemological significance—positive and negative—to intuitions or judgments in general, we erase a theory without thereby replacing it by another. Since we are interested in having *something* determinate to say about philosophical method, we need to refocus and build (more or less) from the ground up.[35] Williamson

[35] As far as I have been able to tell, the positive project in this thesis does not have a methodological precursor. Still, it is not without theoretical background. Many of the thoughts— particularly those pragmatist, rationalist and normativist in character—are inspired by Robert Brandom's *Making it Explicit*, despite Brandom's opposition to the project of conceptual analysis (Brandom 1994: 634-635). Another influence that is closer to directly methodological—one that I did not realize initially—is from James Woodward, and his book on causal explanation (2003). He grounds his argument largely on methodological considerations about the *practical purpose* of an account of causal explanation. It was only after I had begun to express my own methodological thoughts, while re-reading passages from his book, that I saw how important those comments were and how much I agreed with him. Yet later, I

engages in this task by theorizing thought experiments, the counterfactuals involved in them, and the relation they hold to metaphysical modalities. He is concerned with developing an account of philosophical method that does not present it as being exceptional in nature, but as relying on ordinary cognitive skills. Among these, he accords special weight to the ability to assess counterfactuals. While sharing the spirit of Williamson's project—to build philosophical methodology anew—this chapter pursues a different route. The route taken is motivated by a belief in the viability of conceptual analysis when it is properly disentangled from the methodology associated with intuitions and thought experiments. Replacing intuition satisfaction with the satisfaction of a handful of norms, I attempt to articulate fairly determinate methodological constraints for conceptual analysis. The goal is thus to achieve a replenishment of the method of conceptual analysis without reinventing its original purpose. The next chapter defends a presumption of this attempt, which is that a good reason for doing conceptual analysis is to *master* concepts as *participants* of the practice of applying the concepts we analyze. This presumption makes for a significant starting point. It allows us to ask what we have concepts and conceptual analysis *for*, and to answer that question by appealing to the ends we pursue as discursive beings with practical interests. While this chapter is concerned with justifying the norms individually, the next chapter has a general and collective outlook.

The norms I suggest are rational and pragmatic in kind, with the exception of the empirical norm (section 3.2). The aim of the rest of this chapter is to articulate these norms, and argue why they should govern conceptual-analytic practice. The legitimacy of that governance depends on whether, how, and to what extent satisfying the norms is valuable by independent cognitive and epistemic standards. Arguably, the ultimate epistemic values are knowledge and understanding. These set the goals for all epistemic pursuits. To these ultimate values, there are many derivative goals, relating to different kinds of epistemic pursuits. Examples of such derivative goals include predicting and explaining the course of events, explaining the significance of some state of affairs, advancing interpretive skills and skills at reasoning. The norms I

became familiar with Jonathan Weinberg's paper "What's Epistemology For?" (2006), where he outlines a pragmatist "metaepistemology."

suggest relate in their own ways to the ultimate goals. Their value should be apparent in my arguments for them, but it is helpful to keep in mind the ultimate values to guide evaluation of the norms.

I cannot provide a complete characterization of what conceptual analysis is before discussing the norms I take to constrain its proper execution. There is still need for a sketch to that effect, despite the fact that it cannot remain perfectly neutral to the norms I claim should govern it.

We produce a conceptual analysis when we articulate conditions for the truthful applicability of a concept.[36] The intention is to dissect the meaning of a concept into more graspable pieces, so that we gain a better understanding of the concept; so that we can, among other things, better judge what instances fall under the concept and what instances do not. This is clearly a worthwhile project if the concepts we analyze have some practical or theoretical value for us, such as KNOWLEDGE, INTENTIONAL ACTION and CAUSATION. It matters to us whether a belief is true by mere chance or by being a properly justified piece of knowledge; or whether a bodily movement is not just that, but an intentional action; or whether two events are not simply temporally successive, but related as cause and effect. The classical sort of conceptual analysis consists in giving individually necessary and jointly sufficient conditions for concepts such as these.[37] In virtue of their logical form, these analyses license truth-preserving inferences from the applicability of the concept to the obtaining of its necessary conditions, and conversely, from the joint obtaining of the conditions to the applicability of the concept. Hence, conceptual analyses augment our theoretical arsenal by themselves. They can, in extension, improve theorizing in other fields, for instance by making a concept more suitable for application in the special sciences.

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[36] It is somewhat more common to speak of "application conditions" as opposed to "applicability conditions." My preference for "applicability conditions" stems from the idea that a concept is not necessarily applied when it is truthfully applicable (and it is the latter we are interested in). I shall speak of "AC"s for short.

[37] Conceptual analysis by necessary and sufficient conditions is by far the dominant kind of analysis, and for some philosophers it is the only kind of analysis. I do not engage in discussion whether or not anything other than it deserves the label "conceptual analysis," but it will make little difference to the discussion of the norms. Positing only necessary conditions or only sufficient conditions for applying a concept is not a different activity in kind, even if it is theoretically superior to achieve both.

The paradigm example of a conceptual analysis by necessary and sufficient conditions is the traditional analysis of KNOWLEDGE. While most philosophers agree that it is defeated by the kind of counterexample Gettier came up with, it serves as a good illustration of what conceptual analysis is. According to the analysis, *an agent A has knowledge that p if and only if* (1) *p is true*, (2) *A believes that p and* (3) *A's belief that p is justified*. Perhaps this analysis has been given too much attention in methodological discussions, but that is not by chance. The analysis elucidates the most distinctive features of conceptual analyses, even the exogenous feature that they all seem to fail to enjoy general approval when philosophers are given enough time to think about them.

3.1 Statement of rational and pragmatic norms

In the following, I articulate and defend norms for conceptual analysis. Many of them are already quietly or silently at work in philosophical practice. But it is only upon being expressed that a norm can be discussed in proper: only explicit claims can be given reasons (*pro* and *con*).[38] The task of giving expression to these norms is complicated by the fact that there are always many ways of explicating a norm that is implicit in practice. What decides whether a norm is correctly explicated? In line with the normative approach of this thesis (section 1.3), the norms are correctly articulated insofar as they *should* govern conceptual-analytical practice. Whether or not they adequately describe actual practice—in the sense of mapping on to actual argumentative patterns—is not decisive. I still *assume* that most of the norms are descriptively adequate for at least a fair portion of philosophical activity. After all, they are not invented in a void, but there is no direct argument to defend their descriptive adequacy.[39]

There is another consequence of the assumed fact that many of the norms enjoy implicit acceptance in one form or another: some of them will seem fairly obvious. We still need to state them. This is due to the overt negligence of their importance to philosophical practice in the methodological debate about intuitions, thought experiments, etc. The methodological debate developed to the point where it seemed

[38] See Brandom (1994: pp. 79-85), who has inherited this idea from Kant, Frege and Sellars.

[39] This contrasts, as I mentioned in footnote 14, with the project of Cappelen (2012).

as if the only way to justify ordinary philosophical practice were to justify the use of intuitions. The legitimacy of conceptual analysis, it seemed, hinged on the success of intuition in providing us with philosophical truths of whatever preferred sort. But this train of thought was and is mistaken. The legitimacy of philosophical method does not depend on the success of intuition. I claim, to the contrary, that the crucial justificatory demand is that it keep in line with a handful of norms that have been insufficiently appreciated. Because of their apparent neglect, the norms need explication however obvious they might seem upon consideration. Not all the norms in this section enjoy such obviousness. These stand in the need of more argument to meet the dialectical demands. Hence, the norms I discuss form a range from obvious to controversial. I state and discuss the norms in that tentative order.

3.1.1 Internal Compatibility

Starting with the most obvious norm, an analysis should not contain an internal incompatibility between its parts.[40] That is, a concept should not be analyzed into incompatible ACs. If by any chance, for instance, someone finds the need for an analysis of the meteorological concept DRIZZLE, it should not have among its

[40] Regrettably, "Internal *Consistency*" sounds more natural than "Internal *Compatibility*", while "compatibility" sounds more natural in the case of "External Compatibility," the next norm. I have opted to use "compatibility" instead of "consistency" to give the two norms a uniform treatment, and to not confuse the relevant relation with formal/logical consistency. We can, however, understand compatibility in terms of logical consistency. Here is a suggestion, framed in terms of *in*compatibility: First, assume auxiliary premises that link the contents of the two parts of the candidate incompatibility relation. (These premises must be true for INCOMPATIBILITY to be truthfully applicable.) To check for incompatibility: attempt to formally derive a contradiction by assuming both relata and the auxiliary commitments as premises. Clearly, if two propositions or two sets of propositions are inconsistent, they will also be incompatible. But formal consistency does not always get the job done. For instance, we cannot deem the pair of claims "Oslo is to the East of Bergen" and "Oslo is to the West of Bergen" as formally inconsistent, but they are nonetheless incompatible: we could supply the pair with an instantiation of the true premise "If A is to the East of B, then A is not to the West of B." to derive formal inconsistency.

Because checking for compatibility requires auxiliary premises, the ensuing norm will be more controversial than if we had only demanded logical consistency between the original relata. This is quite natural, since more assumptions are *ipso facto* in play. In the ideal dialectic, the auxiliary commitments are platitudes that all interlocutors can agree on; if they are not, they must be argued for, just as with any other claim. I owe my understanding of compatibility to Brandom (1994: esp. 97-105), even if he is opposed to understanding compatibility in terms of formal consistency. (Assuming Brandom's exact concept of "material incompatibility" is too controversial for this thesis, and it is not needed, contrary to what Brandom needs to fulfill his own explanatory obligations: To understand logical vocabulary as making implicit proprieties of inference explicit (Brandom 1994: 97-105).)

necessary ACs (1) that it rains, and (2) that it does not rain. Let us express this as an explicit rule:

> **(IC) Internal Compatibility**: A conceptual analysis must have ACs that are jointly satisfiable.

As cautioned, this might seem obvious.[41] All thoughts must have compatible components to be true, making any thought articulated by a concept with incompatible ACs false.

The reason (IC) is worth mentioning in an account of conceptual analysis' method, is due to the characteristic subject matter, namely, concepts. This requires explanation. Among the most popular theories of the ontology of concepts are those that assert that concepts are mental particulars, in line with "the representational theory of the mind" (see Margolis and Laurence (2011). Fodor (1998) is a leading example). If these theories are right, concepts are nothing more than psychological entities or structures, governed as such by the laws of psychology. Whatever the true laws of cognitive psychology turn out to be, it is highly implausible that a guarantee against inconsistency would be built into them. Humans frequently commit logical fallacies. (Cf. footnote 40, a consistency violation is a compatibility violation by implication.) If conceptual analyses are to truthfully represent and elucidate the concepts we in fact possess as psychological subjects, they should represent them just as they are, whether with compatible or incompatible ACs. On representational theories of the ontology of concepts, then, there is nothing wrong in principle with concepts that have incompatible ACs.[42] Other theories of the ontology of concepts might also allow for such incompatibilities in our conceptual scheme, but the point is clear enough with the representational theories to show why the norm against internal incompatibility

[41] However, there are counterexamples to all of the rational and pragmatic norms insofar as we consider them necessary conditions on the goodness of analysis. The style of example involves analyses that are made for the purpose of uncovering a concept beyond repair, i.e. a concept we should discard. An analysis that does so can be valuable, even if it violates (IC). Several claims going forward are vulnerable to an objection based on this style of example, but I do not resolve its significance until section 4.1. There are no more reminders of the exception in this chapter.

[42] This is overlooking the fact that representational theorists are not typically friends of conceptual analysis, and so do not believe that there are conditions of applicability in the first place (Margolis and Laurence 2003). Rather, they favor the work of psychologists to tell us what our concepts are.

has a special status for conceptual analyses in comparison with other theoretical work: We need extra argument to show why analyses should not have incompatible ACs, when it is possible that they can have such incompatibilities on premises of major theories about what concepts are.

As noted above, a conceptual analysis licenses the inference from the applicability of the concept to the obtainment of its ACs. Clearly, if the ACs of a concept are incompatible, then its applicability implies a contradiction (granted, as we must, that the auxiliary premises needed to derive formal inconsistency are true). But contradictions are not possible. Thus, a concept with incompatible ACs is not applicable. This means that the concept in question can have no truthful use. Therefore, it would be of no epistemic benefit. But concepts should have epistemic benefit; and conceptual analyses should construe them as such. Therefore, conceptual analyses should not be internally incompatible.

Demonstrating satisfaction of (IC) can hardly do any justificatory weight-lifting. But none of the norms in this chapter are supposed to provide sufficient justification upon their lone satisfaction. To see that (IC) is nonetheless necessary, we need only note that dissatisfying it must count as defeat for an analysis.

3.1.2 External Compatibility

Conceptual analyses should be compatible with some properly defined set of scientific theories. That is, analyses should explicate concepts in compatibility with theories that are relevant to the target concept, but patently not in compatibility with all the content-relevant theories ever held true by anyone. Arguably, there is no way to define the right set without resorting to "true" or "correct," or truth- and correctness-implying vocabulary. We could use "well-established," for instance, but that expression would only work insofar as the well-established theories are in fact true and correct. In general, a conceptual analysis should be compatible with any true claim, amongst which true scientific theories are important members. Thus:

> **(EC) External Compatibility**: A conceptual analysis must be compatible with true scientific theories.[43]

As with the previous norm, we could complain that (EC) is too trivial to bother stating. To elucidate, let us assume that conceptual analyses are truth-evaluable.[44] Every truth-evaluable item must be compatible with any other true proposition to have a chance of actually being true. So conceptual analyses must be compatible with true scientific theories to be true. But how are we supposed to employ (EC) in justifying an analysis? It depends on our ability to consult true theories in judging the target analysis. This is no simple matter, since ascertaining whether a theory is true might be equal in difficulty to ascertaining whether the analysis in question is good, and this norm has the latter depend on the former. Thus, the challenge with (EC) lies in its application. It would be preferable if there were a way to make the norm more readily applicable without damaging its status as a necessary condition on the goodness of an analysis. There should be some practical value to writing methodology, even if methodologists cannot be expected to produce "manuals" for performing philosophy. On the other hand, there is no simple way to delete or substitute the truth-clause: we cannot have the compatibility of analyses judged against any old theories, particularly not the false ones.

Boghossian (2005: 211) makes a valuable distinction in this regard, between "objective" and "subjective" norms. Objective norms are the kind of norms that can be legitimate even if they cannot be directly followed. As Boghossian puts it, their

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[43] On how to interpret "compatibility," see footnote 40. A point was omitted in that note, and it should be added here. To ensure that the right pair of propositions or sets of propositions are determined to be incompatible by running the incompatibility test sketched in footnote 40, we must require that the relevant pair of propositions or sets of propositions play essential roles in the inference used to derive formal inconsistency. Thus, any true theory might be needlessly added to the auxiliary premises in an inference intended to uncover *internal incompatibility*, but for the inference to uncover that, and not *external incompatibility*, those theories must be superfluous to the logical structure of that inference. For external incompatibility, however, those theories cannot be superfluous since they are among the candidate incompatibility relata.

[44] In the next chapter (section 4.2) I argue that it is wrong to judge conceptual analyses simply by their truth values, but we can do without this nuance here, for expository purposes. To be sure, the discussion in the main text can be reconstructed in terms of goodness values. When we undertake a commitment (viz. to an analysis) we are precluding our entitlement to hold anything incompatible with that commitment (Brandom 1994: 160). It is certainly not *good* to preclude our entitlement to hold a true scientific theory.

satisfaction need not be transparent to us (2005: 211). Importantly, however, their objective governance does not preclude holding subjective norms—ones that are not objectively valid but can be followed directly—that agents can maintain as means to satisfy the objective norms. Boghossian illustrates by attending to the case of stock market traders. Their objective norm is "buy low, sell high," but it cannot be followed directly. It is only possible to determine which values were "low" and which were "high" in hindsight. Because the norm is impractical, traders instead follow norms related to technical indicators, political stability, fundamental economics, etc. They do this even if the validity of those norms is fully dependent on their subservience of the objective norm. It is similar in the case of satisfying (EC), our objective norm of interest. We do have pretty good ideas about which theories we should maintain belief in, and hence which theories an analysis better not collide with. Moreover, we can flesh out these "pretty good ideas" by way of articulating subjective norms for (EC). One is that compatibility should be maintained with theories that enjoy substantial support from evidence. In fact, all the usual norms for rational belief apply for agents judging whether compatibility with a particular theory is obligatory or virtuous. Metaphysicians are challenged with the theory of relativity in developing or defending a conception of time. If an analysis of TIME or TEMPORAL SUCCESSION is demonstrated to be incompatible with relativity, it is defeated unless it can show that the theory of relativity is false, or show that it is not really incompatible, for instance by proving that the auxiliary premises needed to demonstrate incompatibility are false.

Generally, concepts are open to scientific revision, and philosophers analyzing concepts are best advised to have their analyses be compatible with the theories of prevailing science, unless they can prove that science wrong, or show that their claims are not actually incompatible. To illustrate the potential for scientific concept determination, consider Putnam's Twin Earth scenario (Putnam 1973). On Twin Earth there is a water look-alike substance with chemical composition XYZ. Twin earthlings refer to it by uttering "water." If one of us were to visit Twin Earth, however, we would be speaking falsely if *we* said of the XYZ substance that it is water, since what *we* use "water" to refer to is the substance with chemical composition H_2O. Thus, the meanings of our words, i.e. the conceptual contents associated with the expressions we use, are not wholly determined by our individual

psychological states. To all non-scientific appearances, the XYZ substance is identical to water, so our psychological state upon seeing it would be just the same, had it in fact been water. The determination of meaning is not just a psychological, "internal" matter; it is also social. Among other things, we are part of a linguistic community whose scientific progress has allowed us to identify water with H_2O, thereby fixing the applicability condition for the concept WATER to the applicability of the concept H_2O.[45]

Scientific development is one of the most important sources of conceptual formation. This is due to the precise observation and experimentation that goes into scientific practice, as well as the rigorous demands it stands under for systematicity, explanatory strength and applicability. These sharpen its conceptual framework and enable scientists to make such precise observation and experimentation in the first place. Hence, it is important that analyses are construed to be compatible with prevailing scientific theories if the analysis shares with one its subject matter, or if the scientific theory has implications for the subject matter of the analysis. The point stands even if philosophers are not typically in the business of analyzing natural-scientific concepts, such as WATER. To illuminate with an example more to the point, an analysis of RATIONAL ACTION should not use DESIRE in ways that are incompatible with the best psychological theory, unless, that is, the theorist is willing to commit to be speaking of something else than psychological desire (in which case a great deal of explaining is owed to the readers) or willing to demonstrate why the leading psychological theory is wrong.

3.1.3 Understandability

The two preceding norms are more practically relevant for arguing *against* the goodness of analyses, than being norms whose satisfaction it is proper to invoke in positively justifying an analysis. Their dissatisfactions are more remarkable than their satisfactions. Theorists are impugned when they commit logical fallacies, but they are not expressly praised for the logical consistency of their work. Similarly for the above

[45] It is worthwhile to remember that the deliverances of science are not the only sort of "external" determination of meaning. Even in Putnam's hypothetical scenario we can spot another sort, particular to fictional and hypothetical writing. In setting up the hypothetical scenario, Putnam fixes the meaning of the twin earthlings' use of "water" to XYZ.

norms. Because we are limited by space and time, it turns out to be fastidious to point to every positive justificatory feature of a piece of work, even in a careful and thorough practice like philosophy. That does not mean that the norms governing those features do not legitimately govern them as such. But other norms are more likely to be among the crucial points in the positive justification of an analysis, such as the following:

> **(U) Understandability:** A conceptual analysis should advance our understanding of the target concept by supplying ACs that are, pre-analytically, better understood than the concept itself.[46]

In Boghossian's terms, this is clearly an objective norm. We can interpret it as a superordinate rule: It contains, as it were, some of the more detailed norms I discuss subsequently. This feature is connected to the fair share of obviousness I take it to enjoy: That conceptual analyses ought to serve our purpose of gaining understanding can be considered a primary goal of conceptual analysis.[47] If not to get a better grip on the concepts we are interested in analyzing, why bother? Since gaining understanding is such a basic goal, (U) is at once loaded with assuming vocabulary, as well as being among the norms whose legitimacy is not really in question.

The price to pay for obviousness is again—recall (EC)—that the actual applications of (U) will be contentious. We can agree that analyses ought to benefit our understanding, but we have not thereby agreed on which analyses in fact do so and *how much* benefit they confer. Admittedly, to ask which one out of a set of analyses increases our understanding of a given concept the most can be interpreted as simply asking which analysis is correct. The intention behind (U) is not to rephrase that even more obvious demand. Rather, there are certain structural features of conceptual analysis that contribute to our understanding of a given concept, regardless of what *particular* content that concept is loaded with. To clarify this, and to make the

[46] Cf. footnote 5: I use "pre-analytical concept" to denote the concept as its content is given in absence of the analysis. The corresponding phrase is "analytical concept," to denote the concept as its content is given by the analysis. These expressions are used frequently later in this chapter.

[47] Cf. Elgin (2006). Elgin argues that epistemologists have overemphasized KNOWLEDGE at the cost of neglecting UNDERSTANDING. She argues that UNDERSTANDING is much more relevant to apprehend the epistemic benefit of science. If so for science, it must be all the more relevant to comprehend the epistemic benefits of philosophy.

application of (U) more determinate, we need to assess the more detailed norms that subserve it. In this errand I articulate the norms for conceptual integration and determinacy. We gain understanding of a concept by producing an analysis that advances the level of integration in our conceptual scheme, and by making the concept's applicability more determinate. However well these norms subserve (U), it would be unnecessarily brash to put them forth as the only possible ways by which analyses can benefit our understanding. Whereas (U) stands on independent grounds, the following two norms depend on (U) for their legitimacy.[48] In case the norms for determinacy and conceptual integration do not exhaust the content of (U), the latter serves as a reference point for developing other subservient norms.

3.1.4 Conceptual Integration

The norm for conceptual integration requires some pretext, so here is a plausible idea expressed to serve that purpose. Considered in its entirety, we have a conceptual scheme that includes all the concepts we ever apply, entertain applying, disagree about, etc., and a central feature of this scheme is that the concepts it comprises are connected by links of various degrees of tightness.[49] One way to flesh out this idea is that the concepts of the scheme are connected to each other by being inferentially related with the backing of implicit and explicit theories involving the concepts.[50] We have explicit physical theories, for instance, relating MASS to ENERGY, TEMPERATURE to MEAN KINETIC ENERGY, WATER to H_2O; biological taxonomy relating WHALE to MAMMAL; philosophical theory relating KNOWLEDGE to JUSTIFICATION, and so on. We also have implicit theories (i.e. "folk theories"), linking most of the concepts in the

[48] This is not entirely true. The legitimacy of the norm for determinacy does not solely depend on its subservience of (U), but also on its contribution to explanatory utility (see section 3.1.6).

[49] Talk of a "conceptual scheme" lends itself to the unwanted ideas that our conceptual scheme could have been *radically* different, and to what Davidson calls a scheme/content dualism (Davidson 1984). Kant, as we shall see below, ends up with such a dualism, by thinking of concepts as conceptualizing the unconceptualized. These are not necessary implications of holding that we have a conceptual scheme, however, and they are not implied by the claims I make about it. Cf. footnote 72.

[50] The familiar idea I am proposing here is expressed in a manner suggesting we should assume an inferential role semantics, but there can be other ways to flesh out the idea that concepts are related to each other— in various degrees of strength—that do not suppose that their *fundamental feature* is the inferential relations they bear to other concepts.

grand scheme, e.g. HUNGER to EAT, RED to GREEN,[51] SMALL to SIZE, and so on. Many of these conceptual links sustain in virtue of the joint backing of explicit and implicit theories.

To get an impression of the complex interrelations involved in our conceptual scheme, we can visualize it as a great web. The strings holding the web together are unevenly distributed: some areas are tightly knit together while being connected to the rest of the web by just a few strings, some areas are located in central parts of the web, other areas do not give an impression of systematic connections at all. Now, imagine that the web is in fact a spider's web, only that there is not one, but a swarm of spiders moving back and forth on the web. As they move by a string, they let off some more silk, strengthening the string in the process. Occasionally, some of the spiders bring about new strings by connecting previously unconnected parts of the web; other times cutting off a string or two is needed to make way for a more robust structure. Sometimes they bring about whole new parts.

The point of imagining an *inhabited* web is to remind us that our conceptual scheme is not set in stone. It is constantly evolving because it is *we* who maintain it. We do this by employing the concepts we have at hand, both in new ways and in our reinforcing of old ways, and our occasional development of new concepts.[52] It is important for the project in this thesis that we recognize that the conceptual scheme is ours to maintain and develop through norm-governed activity. More to the point of the norm in this section, the strings of the web are unevenly distributed, and some parts are more central than others. These properties translate to the facts that our conceptual scheme is grouped into spheres of interconnections between concepts and that these spheres exhibit a stratified structure. Some concepts are more basic than others. As such, they are more crucial in upholding the scheme's structure.

[51] I.e. the links are not just of the form "A$x \supset$ Bx." They can be complex in form, and may involve a host of other concepts, thereby linking them as well. (For instance, cf. the sort of "Ramsey-Carnap-Lewis-Jackson" style "network analysis" Michael Smith (1994: 44-56) considers and rejects for moral concepts. He also rejects them for color concepts, but they can work if they do not come with a demand for reduction.)

[52] Nativists contend that most of our concepts are *innate*, which precludes that they have evolved through the course of a living discursive practice. I am writing from a different theoretical perspective, and have not made sure the norms are compatible with nativism. They might be, but I have not attempted to vindicate that possibility. See Margolis and Laurence (2011) for more on nativism.

Furthermore, since concepts are made intelligible in terms of each other, we do not want parts of the conceptual scheme to go unconnected with the rest. As far as possible, it is favorable that the scheme has a unified structure from basic concepts to more complex ones, where the latter are made intelligible by the former. To see why this is favorable, consider the improvements scientific development has made to the integration in our conceptual scheme. In the last few centuries, substances familiar to the everyday perceiver were discovered to have specific chemical structures, and these were in extension related to atomic and subatomic structure. Thanks to these developments, the level of integration in our conceptual scheme—specifically, the sub-parts of the scheme concerning the physical world—made huge leaps. We could thereby grasp the commonality between everyday substances by the differences in the structure of common, sub-microscopic particles. No matter if we conceive of these developments in purely realistic terms, as discovering the fine structure of the physical world, and no matter if the relevant relations between concepts are cashed out differently (e.g. by causal-functional relations, instead of the normative inferential relations I have assumed), the consequences such scientific development had for the structure and relative integration in our conceptual scheme are undeniable.

Before defending its legitimacy, we can state conceptual integration as a norm for conceptual analysis:

> **(CI) Conceptual Integration:** A conceptual analysis should increase the level of integration in our conceptual scheme.

With (CI), the crucial interpretive issue concerns the word "integration." The pretext offered above should give at least a vague pointer as to how there can be such a thing as conceptual integration, but we need some deeper and more specific explication to appreciate the significance of this norm.

The norm is asserted in the spirit of the so-called Ockham's Razor. The most familiar expression of it—although it has not been found in the writings of actual William of Ockham—is, "do not multiply entities beyond necessity" (Spade and Panaccio 2011). Ockham was a nominalist metaphysician, and the primary application of the principle is in ontology, to favor parsimonious ontologies over inflated ones. Ockham's Razor is also interpreted as recommending the theory with the fewest assumptions—

whatever its subject matter—provided that the theories in the comparison class are equal in other regards. It might be tempting to regard this as a truth conducing principle; that following it should somehow increase the probability of hitting truth. Regarding it that way, however, is needlessly narrow sighted. Truth is not the only kind of theoretical virtue, and we disadvantage ourselves in not acknowledging other kinds. In the following, I think it is shown clearly that the force of Ockham's Razor is due to the practical necessity of making things intelligible, i.e. of enforcing the virtue of understandibility.

To comprehend what is at stake with the application of Ockham's Razor to conceptual analysis, we must consider what fundamental service concepts have in our epistemic lives. This is patently a Kantian theme, and rehearsing Kant's view will provide us with a clue to the issue. Kant held a classificatory view of our cognitive apparatus. What we do, according to his view, is categorize particular "intuitions" (*Anschauungen*), by "synthesizing them according to a rule" (Kant 2005: A79/B105, A159/B198).[53] Concepts are precisely these "rules for synthesis." They determine the content of our experience. In so doing, concepts have our experience organized, instead of having it be a chaotic flashing of unconceptualized "givens." As Kant accordingly held, there would be no experience proper without the synthetic activity of our conceptual faculty. A perception without a concept is blind. Without our conceptual faculty, then, we cannot *grasp* the sensorily mediated content presented to our consciousness; that content would mean nothing to us.[54]

We can extract two points from Kant. First, that it is essential to concepts that they somehow unify. Second, that epistemically significant intentional states are not possible without concepts. While concepts are essentially re-applicable, particular sensory representations of the world are essentially non-repeatable. In some sense

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[53] "Intuition" in this context is not what the previous chapter offered a conception of. In Kant translations, "intuition" is closer to "sensory affection" than to those propositional attitudes. The quote-marked phrase is not a direct quote, but an abstraction formed with Kantian vocabulary.

[54] In a Kantian vein, but from a different angle, Hegel also argues for the necessity of conceptual ordering of our experience in the first part of *The Phenomenology of Spirit* (Hegel 2009: Part A, "Consciousness"). Hegel assumes the point of view of a consciousness that supposedly experiences its object unaided by conceptual articulation, but reveals, from the inside, the failure of such "experience" to hold epistemic significance.

then, concepts "contain" a manifold of represent*eds* (possible and actual) in a single representational unit.[55] They "contain" their manifold instances by being repeatably applicable to them. While concepts are not simply labels for whatever they apply to—for one thing, we attach practical values to the applicabilities of concepts; for a second thing, concepts play roles in determining what inferences are correct—it is essential to their service that they unify representations, in the sense of making our would-be epistemic lives not just a collection of scattered sensory happenings.

So what is the relevance of the indispensible unifying function of concepts to (CI) and Ockham's Razor? If an essential and constitutive purpose of having concepts is to unify, we can extend the argument for having concepts in the first place to show that we should strive to unify the concepts themselves, which is to say that we should strive to integrate our conceptual scheme. If the reason for having concepts in our coping with the world (and our coping with our coping with the world), is to enable us to represent something beyond non-repeatable particulars, it seems that having an infinite number of concepts—each to capture the particularity of a single experiential representation, or something along those lines—would be of no help. Because the point of having concepts is to contain in a single thought something that extends beyond what could be contained in a single non-conceptualized "experience"—viz. in a particular sensory stimulation—the same epistemic worthlessness that infects non-conceptual "experiencing" must not infect the conceptual scheme itself. It is practically impossible to manage an infinitely complex and disconnected conceptual scheme. Assume that we have such a scheme, and want our thoughts to equally well express and represent what we can express and represent by articulating thoughts with our actual, fairly well-integrated conceptual scheme. In that case, we would have to articulate infinitely complex thoughts. But we are finite beings; we cannot comprehend infinitely complex thoughts.

We can now appreciate that the legitimacy of (CI) (and, derivatively, Ockham's Razor) is due to the practical necessity of meeting the needs of our finite understanding. By carefully applying the razor to our conceptual scheme, we intend to

[55] It is necessary to use "representational unit" instead of "representation" to not overlook Kant's basic insight that the only use of concepts is to form judgments (Kant 2005: A68/B93), and so that concepts are not complete representations by themselves.

allow single, comprehensible thoughts to express more and to represent more. As long as our conceptual scheme is not so parsimonious as to obstruct our need to take in the full richness of the world, we need to limit the tools with which we represent it, or else we would do just as fine without any concepts. (CI) translates this to a methodological norm for conceptual analysis as it demands that analyses contribute to the integration of our conceptual scheme.

The foregoing, abstract thoughts do not suffice to make (CI) a useful norm, even if they give it a theoretical foundation, and are the considerations, I believe, that rightly justify it. We also need to see how the norm can be applied more concretely. One way in which conceptual integration is effected, then, is when a concept is analyzed in a way that lets us understand the commonality between disparate applications of the concept. For instance, in the previous chapter, I argued that the etiological conception of intuitions—the one that holds that intuitions are expressions of conceptual competence—had our concept of intuition needlessly disintegrated. The conception obstructs our view of what "rational" and "physical" intuitions have in common, which I later argued was a dialectical role and an unsettled strength of assent. Distinct and disconnected concepts of rational intuition and physical intuition were "entities beyond necessity." This is not a matter of limiting the sheer number of concepts in our conceptual scheme, but of not multiplying the concepts we treat as basic. Analyses that do not bring together disparate applications of a concept do not satisfy (CI). The exception is when these applications necessitate different concepts; that the applications in question turned out to be non-unifiable in the light of other, more important commitments.[56] What I argued in the previous chapter was that disparate applications of the concept INTUITION do not necessitate different concepts.

Perhaps it is easier to appreciate (CI)'s significance by considering another sort of dissatisfaction: the overtly circular analysis. An overtly circular analysis has no concepts expressed in the *analysans* that are not already in the *analysandum*. That is, all the analysis does, is analyze the target concept in terms of itself. An analysis of CAUSATION is circular in this way if it asserts, "*c* causes *e* if and only if *c* causes *e*." This violates (CI) by not relating the *analysandum* concept with any other concepts,

[56] For instance, save violating (IC), there might be no combination of ACs for a single concept that makes its disparate applications collectively intelligible.

no mind relating it to more basic concepts or concepts that can be understood independently of it. Overtly circular analyses do not satisfy (CI).

While the overt kind of circularity is definitely vicious, it is less definite where the line between vicious and permissible goes for analyses that are only *covertly* circular.[57] A conceptual analysis is covertly circular if a concept in the ACs in some sense presupposes the concept analyzed. If that occurs, neither the *analysandum* concept nor that *analysans* concept can be understood except in terms of the other. This is the case when a concept with a well-established analysis, or a well-established necessary condition, is employed in analyzing a concept that figures in its well-established analysis or the well-established necessary condition. For example, the truth condition on KNOWLEDGE is a widely accepted necessary condition. In asserting it, we are purporting to make KNOWLEDGE intelligible in terms of TRUTH. On this background, it would be circular to analyze TRUTH in terms of KNOWLEDGE, for instance by asserting that a proposition is true just in case the proposition is the content of a knowledge state of a possible or actual agent. In terms of the web metaphor, this would only count as moving back and forth on the same string.

In taking a stand against the permissibility of such analyses on premises of the conceptual integration they effect, it is not sufficient to demand that the *analyzed concept* be integrated. Clearly the concept in a covertly circular analysis *is* being integrated, albeit with the help of a concept that already presupposed it. Hence, we cannot restrict our attention to just the analysis in question if we are to determine whether it is viciously circular. That is why (CI) was stated as demanding that the analysis increases the level of integration in our conceptual *scheme*, instead of the weaker demand that the analyzed concept be integrated, regardless of how the analysis ties up with the rest of the scheme. It matters that we already hold KNOWLEDGE to be intelligible in terms of TRUTH, when we judge whether the above analysis (of TRUTH in terms of KNOWLEDGE) is circular.

The label "covertly circular" hides finer-grained differences between its instances. The degree of integration effected by covertly circular analyses are not equally lacking. So, if we take the degree of integration that circular analyses effect as a

[57] I owe the distinction between overt and covert circularity to Humberstone (1997).

measure of their permissibility, then not all those analyses are equally vicious. This depends on whether all of the *analysans* concepts presuppose the *analysandum* concept, or if just one or some of them do. It is also crucial what role the concepts that presuppose the *analysandum* play in the ACs.[58] If they do not play the central-most role in making the target concept intelligible, the analysis in question need not fail (CI). Despite presupposing the analyzed concept at some level or in some way in the ACs, then, these analyses do not cross the boundary for being unacceptably circular.

The best way to see that there can be permissibly circular analyses is by example. Consider a manipulability theory of causation (specifically, Woodward's (2003), to which we shall return in section 3.1.6). The theory analyzes CAUSATION in terms of the changes an actual or hypothetical intervention on a candidate cause would make to its candidate effect. On this account, the relata of the causal relationship are treated as *variables*. They have actual values, associated with whether and how the events actually occurred. When the cause-variable is intervened on, it attains a different value than its actual value. While there are important details about precisely what interventions, or combinations of interventions, we must carry out to test a causal claim, the basic idea is that there is some intervention on the candidate cause that would also result in a different effect. The analysis is covertly circular. We cannot understand what INTERVENTION is without the concept CAUSATION. Intervening is causing things to be different than what they would be.

However, notwithstanding the analysis' use of INTERVENTION in the ACs, it is primarily a counterfactual analysis. The analysis holds that, when we claim that *C* causes *E*, we are claiming that some values of *E* counterfactually depend on some values of *C*. It is in specifying how *C* attains those alternate values that we must employ the concept INTERVENTION. An intervention on *C* is giving it a specific value by causing it to attain that value (and the central question is, if that would change the value of *E* as well). Thus, the analysis does not have us presuppose the obtainment of the particular causal relationship *we are inquiring about*. That relationship is to be

[58] Admittedly, the wording of (CI) might not capture such a nuance. All it demands is *that* integration of our conceptual scheme be effected, not *how*, which might be required here. I.e. this is one of the points on which my precaution above—that the norms for conceptual integration and determinacy might not exhaust (U), in which case we should develop other norms—could be relevant.

made intelligible by the counterfactual dependency relations between the values of the variables that represent the candidate causal relata. Even if the analysis presupposes our understanding of CAUSATION, since we cannot determinately apply the ACs without knowledge of *other* causal relations—at any rate, the causal relation between the intervention and the cause of interest—this circularity does not involve the relation whose obtainment is the target of inquiry.

So, the manipulability analysis employs a concept (INTERVENTION) in analyzing another concept (CAUSATION), but cannot make the former intelligible without the latter. Hence, the integration this analysis effects to our conceptual scheme, if any, is not due to that relation. It is rather the employment of the concept COUNTERFACTUAL DEPENDENCE that does the integrational work. Importantly, this is also the primary analyzing concept, in the sense of being the concept that directly reflects the relationship between cause and effect in the ACs. Our understanding of COUNTERFACTUAL DEPENDENCE—as opposed to INTERVENTION—does not rest on a prior understanding of CAUSATION. While Woodward demands that causal facts must be built into the *antecedent* of the counterfactual that represents the causal relationship in question, understanding the counterfactual dependency *relationship* does not require a prior concept of causation. *That* relationship could be equally well understood without causal information in the antecedent. However, if the antecedent does not contain causal information, the counterfactual will not correctly represent a causal relationship, since the concept of an intervention is needed in the antecedent of those counterfactuals. Since the primary analyzing concept does *not* presuppose the analyzed concept—and since the analyzing concept that *does* presuppose it plays a circumstantial role—the analysis is not circular in a way that has it violate (CI), at least not in a major way. The manipulability analysis advances the integration of our conceptual scheme by allowing us to grasp CAUSATION by the non-presupposing concept COUNTERFACTUAL DEPENDENCE.[59]

This argument shows that we must apply the concepts of circularity and vicious circularity with care. Even if an analysis exhibits a circular trait, it need not be defect.

[59] This reading of Woodward may be controversial. See Woodward (2003: 136) for his own thoughts on the matter. For the purpose of discussing (CI), it is not necessary to evaluate the soundness of my interpretation in detail.

It depends on the specifics of the circularity in question; how central it is to the analysis and whether the analysis has important non-circular traits as well. (CI) gives us a way to substantiate these kinds of judgments, even if it does not settle a perfectly determinate criterion for when we should approve of analyses in the light of their circularities, and when we should reject them.

3.1.5 Determinacy

A good reason for producing a conceptual analysis is to determine which instances the concept in question is truthfully applicable to. In asserting determinacy as a legitimate norm for conceptual analysis, I contend that we should interpret that literally. We want to ascertain precisely which instances do, and which instances do not, fall under the concept in question. But determinacy comes in degrees. Concepts can be more or less determinate. It is when we consider the most extreme cases of indeterminacy that we most easily see why determinacy is a valuable feature. If a concept is wholly indeterminate, it is simply not applicable. But concepts are epistemically worthless unless they are applicable. So some degree of determinacy is necessary for the epistemic worth of concepts.

If determinacy is necessary for the epistemic worth of *concepts*, it is also necessary for the ACs of an *analysis* to be determinate. So if an analyst articulates ACs for a concept that are more determinate than what the analyzed concept would be to its users in absence of the analysis, he has clearly achieved something of epistemic merit, provided the steps needed to make the ACs determinate does not have the analysis fault more severely in other regards.[60] Hence:

> **(D) Determinacy**: A conceptual analysis should have ACs that are at least as determinate as the pre-analytical concept.[61]

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[60] Cf. footnotes 5 and 46: "in absence of the analysis" = "pre-analytical."

[61] Preferably, the ACs are *more* determinate than the pre-analytical concept—increasing the determinacy of a concept is a rightful goal according to the argument of this section—but the norm is stated with the weaker "at least as" as one part of a concession to potential conflict with other norms. Specifically, if the ACs are sharpened to determinacy levels beyond that of the pre-analytical concept, that sharpening can co-involve violating the demand that the analysis ought to conserve the content the concept has attained in practice (cf. the empirical norm, section 3.2). The other part of the concession to this potential conflict, is that the norm is stated with "should" instead of "must." Cf. section 4.1.

We can settle whether this norm is satisfied in a given case if we can settle the two relevant determinacy values, 1) how determinate the target concept is (pre-analytically) and 2) how determinate its candidate ACs are. Unfortunately, I have no special theory of determinacy; I do not provide a metric with which to judge these determinacy values, except from that already assumed in calling some contents determinate and others indeterminate. I am relying on a rough-and-ready, pre-theoretical understanding of determinacy, and I assume we can make do with such an understanding. A few words about what determinacy is and a couple of examples to elucidate are still in order.

The determinacy of a concept or a proposition is reflected in the degree to which we can, without hesitance or contention, apply the concept to, or assert the proposition of, whatever it is we consider applying the concept to or asserting the proposition of.[62] We can grasp what is involved in this by considering *in*determinacies. The indeterminacy associated with concepts that have borderline instances, e.g. BALD, is a paradigm kind. To make this kind of indeterminacy vivid, we can imagine an indefinite series of people that are ordered from the person with the most hair on his or her head, to the person with the least. BALD will be definitely applicable to some extension of people in the latter part of the series, and definitely not applicable to some extension of people in the former part. For many people in between those extensions, it will be undecided whether or not they are bald. They will be borderline instances. As such, we will hesitate to apply BALD to them, even if our understanding of BALD is not incomplete. The hesitation has to do with the fact that our common concept of baldness is not perfectly determinate. But indeterminacy need not be manifested in borderline instances. For instance, it is indeterminate whether or not a relationship between two people counts as being symmetrical, unless it is specified in what respect it is supposed to be symmetrical. The relationship can be symmetrical with respect to their individual economic power and not be symmetrical with respect to emotional involvement. Similarly for SIMILAR. It is indeterminate which of A and B is more similar to C, unless the respect of similarity is specified. In contrast with BALD, there are no borderline instances that manifest the indeterminacy of these

[62] The wording in this sentence should not mislead us into thinking that concepts can be applied outside of propositions. Concept applications can be sub-sententially expressed, e.g. “Red.”, but those expressions should be understood as elliptical expressions of the relevant proposition, e.g. “*x* is red.”

concepts. The problem is rather that we do not even know what to look for, to determine whether a relationship is an instance of SYMMETRICAL or a pair an instance of SIMILAR (unless the respect of symmetry or similarity is specified or presumed). Despite the clear difference between the former and the latter kind of indeterminacy—which suggests the possibility of a taxonomy—I cannot safely say that there are not more kinds. We are better served by focusing on particular instances of indeterminacy as we encounter them.

Why, then, should an analysis provide ACs that are at least as determinate as, and preferably more determinate than, the pre-analytical concept? An argument can be made to show that this is far from obvious. Consider again BALD. We can articulate a tentative analysis for it along the lines of (b) *someone is bald if and only if they have little or no hair on their scalp*. The AC provided here is not determinate. It does not tell us *exactly* how little hair is sufficient to be an instance of baldness. Compare with the following analysis, (b*), *someone is bald if and only if the number of hair strings on their scalp count fewer than a sixth of the maximal number of hair strings they have had at any one point in their life* (for purposes of applying BALD on this criterion, suppose that the maximal number of hair strings can be identified by some scientific technique). Surely, there is nothing wrong with such a stipulative move *per se*. Say, there could be a (dubious) sociological study inquiring whether baldness correlates with certain habits, in need of an operationalized variable. But it would take more argument to convince us that anyone applying BALD is applying it wrongly if their application dissatisfies (b*). Our common concept of baldness seems to be better reflected in (b) than (b*). Therefore, it seems that (b) is a better analysis than (b*), contrary to what the norm for determinacy recommends.

I have three responses to this argument. First, it should be noted that (D) is officially compatible with the goodness of both the analyses, not just (b*). What it demands is that the ACs be *at least as* determinate as the pre-analytical concept, and BALD is no more determinate than LITTLE OR NO HAIR. Still, this response does not resolve the issue of whether it is right that an analysis' ACs are *preferably* more determinate, which is my addendum claim.

Secondly, then, there is a more subtle mistake in the argument, located in the final move. This is from the proposition that our common concept of baldness seems to be

better reflected in (b) than in (b*), to the proposition that (b) is a *better analysis* of BALD than (b*). This move is highly problematic. Unless the argument is refined to incorporate other factors, this inference presumes that the norm to conserve content (section 3.2) is the only norm with which to judge the goodness of an analysis; that an analysis is justified only insofar as it captures the pre-analytical content of a concept. Meanwhile, our acknowledgment of other goodness determining factors, such as determinacy, does not commit us to holding that (b*) is a better analysis, or even simply a good analysis. That is, the conclusion may still hold in the light of all the rightful norms for conceptual analysis. However, that does not take away from the fact that the determinacy of (b*) is a valuable trait, and that it ought to be factorized when we make a comparison of goodness between (b) and (b*).

Third, the fact that (b) scores low on determinacy and still seems like the better "analysis" of BALD is better explained from the fact that an analysis of BALD is not needed—that the concept is inapt for a proper analysis. Despite its indeterminacy, we do apply the concept BALD with communicative success. The practice of ascribing baldness does not hinge on sharp distinctions between instances and non-instances. This, in turn, can in be explained from the fact that we do not attach high practical or theoretical value to whether someone is bald or not. It does not really matter. This feature contrasts sharply with the value we associate with the applicability or inapplicability of the concepts philosophers are more typically in the business of analyzing, such as JUSTICE, RESPONSIBILITY, PERSONHOOD and KNOWLEDGE, for which sharp distinctions are wanted. Subjecting an explication of BALD to the stricter analytical demands—those we subject explications of these concepts to—would be misguided. This judgment is reinforced from the fact that if we were to somehow find ourselves in need of an analysis of BALD, that would likely be a circumstance in which we suddenly *would* have an interest in a sharp distinction between instances and non-instances, such as the case would be for the sociologists hypothesized above. The above argument confuses us, since it does not distinguish what would be a good explication of the concept BALD to someone unfamiliar with it, with what would be a good analysis of it. Conceptual analyses can be governed by the norm for determinacy even if dictionary-esque definitions are not so governed.

I believe these or similar reasons could be given to respond to any argument attempting to show that (D) is an illegitimate demand, that goes by way of considering tentative analyses for indeterminate concepts. As I argued, however, the practically and theoretically important concepts, i.e. the more usual suspects of analysis, are in stronger need of sharp distinctions. If these are not determinate—and if they are not given determinate analyses—it will be arbitrary whether they are correctly applied on a given occasion. The value we associate with the applicability of these concepts does not square well with such arbitrariness. Should there be a point in giving analyses and not just dictionary-esque definitions, we ought to keep subjecting analyses to strict demands such as (D), even if that precludes some concepts from analysis. Hence, we can reassure ourselves that (D) is a legitimate norm for conceptual *analysis*.

We shall now consider a genuinely philosophical dissatisfaction of (D), namely, Lewis' counterfactual analysis of CAUSATION (Lewis 1973a). This analysis is particularly interesting as a case study since Lewis provides methodological considerations in defending the analysis, in the light of its indeterminacy.[63] According to the analysis, an event *c* causes an event *e* if and only if either 1) *e* causally depends on *c*, or 2) there is a chain of events, in which *c* is the first event and *e* is the last event, such that each successive event depends causally on its immediate predecessor. Of course, this does not mean much without a word or two about CAUSAL DEPENDENCY. Lewis analyzes it thus: an event *e* causally depends on an event *c* if and only if the two counterfactuals

$O(c) \mathrel{\square\!\!\rightarrow} O(e)$ and

$\neg O(c) \mathrel{\square\!\!\rightarrow} \neg O(e)$

are true, where O(*c*) and O(*e*) express, respectively, the propositions that *c* occurs and that *e* occurs (Lewis 1973a: 563). That is, the two counterfactual claims, *if* c *had occurred,* e *would have occurred,* and, *if* c *had not occurred,* e *would not have*

[63] Note that the analysis is only indirectly indeterminate. What the indeterminacy in question really haunts is any reading of *counterfactuals* that rests on an overall similarity ordering of worlds. Since my own reading experience of counterfactuals is mainly limited to their application in analyses of CAUSATION, I have chosen to treat Lewis' analysis of CAUSATION as the locus of indeterminacy, despite being indeterminate only in virtue of the counterfactual semantics it employs.

occurred, must be true for the causal dependency of *e* on *c* to obtain. So far, so good. What then, is involved in assessing these counterfactuals? If we consider only the first counterfactual (apply the appropriate negations to get at the second), one of the following two conditions must obtain. A) There is no world wherein *c* occurs (in which case the counterfactual is vacuously true). B) There is no possible world w_1, wherein *c* occurs and *e* does not occur, that is **closer** to the actual world w_a than the **closest** of possible worlds w_2 wherein *c* occurs and *e* occurs. Finally, we can point to the indeterminacy of the analysis, which is one Lewis readily admits (1973a: 557, 559-560). It is not determinate which of w_1 and w_2 is closer to the actual world w_a.

The interpretation Lewis gives of what it means that a world is closer to the actual than another is by way of an ordering of worlds according to an *overall similarity relation*. As Morreau (2010) has shown in detail, and as Lewis was well aware of in 1973, the overall similarity relation cannot be determined. Similarity orderings are possible to determine for single respects, and at best for a limited number of respects, by using a finite weighting function. We can, for instance, determine which of two people are more similar to a third person with respect to height or income. But for infinitely many respects, it is not possible to determine whether A is more similar to C than B is. Considering only a limited number of respects, we can see why not. Say it is raining in a spatio-temporal slice of both w_1 and w_a, while not so in the counterpart slice of w_2. Say also that in both w_2 and w_a, a powerful politician is considering an ingenious idea with major social consequences, while he is not in w_1. How are we to determine which of w_1 and w_2 is closer to w_a? Saying that it is the world that is more similar overall gets us nowhere. The similarity orderings needed to assess causal claims on premises of Lewis' analysis cannot be determined.[64]

[64] At this point, a sharp reader might want to argue that I am confusing semantic issues with epistemological issues. The claim goes that Lewis' similarity ordering is perfectly determinate, and that it gives the right semantics for counterfactuals as such, only that it is a difficult epistemological question how we can know anything about these orderings. But the objection is ill-founded. First, there is no way to judge the determinacy of anything unless that determinacy is epistemically accessible. In case something can be determinate in a fully epistemic-transcendent sense, there is no use for that determinacy. Thus, that is not the sense by which (D) demands determinacy, since (D) is funded by practical, discursive concerns. Second, the objection alienates the audience of semantic accounts. *We* are the audience of semantic explications, and there is no reason to give such an explication—be it analytic or dictionary-esque—if the semantic interpretant is not epistemically accessible. Naturally, many things are epistemically accessible only on a *background* of auxiliary premises, such as scientific

We shall soon see why the above indeterminacy does not render Lewis' causal analysis bad, but first we must establish that its indeterminacy is in fact a weakness. As I have noted, Lewis acknowledged the indeterminacy of the overall similarity relation already in the 1973 paper. What he did not acknowledge was the adequacy of (D) as a criterion to judge the goodness of analysis. He did not think his analysis had to be determinate: "Must [a counterfactual] assign sharply determinate truth conditions? […] Then [no understanding of counterfactuals] will be forthcoming. So much the worse for those standards of adequacy." (Lewis 1973a: 556) In fact, he went even further and claimed that an analysis of CAUSATION *should* be indeterminate: "But the vagueness of over-all similarity will not be entirely resolved. Nor should it be. The vagueness of similarity does infect causation, and no correct analysis can deny it." (Lewis 1973a: 560). Note the use of the word "infect" in speaking of vagueness. Despite claiming that it cannot be resolved for analyses of CAUSATION, and neither should be, Lewis apparently views the indeterminacy as undesirable. We can find more support for this claim. Not for nothing does he argue that we do make overall similarity comparisons, that these are not shots in the dark, and not for nothing does he give pointers on how to weight similarities (Lewis 1973a: 559-560; 1979: 472).

Lewis is right in saying that indeterminacy *infects* his analysis of causation, and he is right in trying to resolve the indeterminacy by offering weighting principles in a later paper (Lewis 1979: 472). Naturally, the reason he is right in doing these things is that the ACs of an analysis should not be indeterminate. As I have argued, indeterminacy hinders our ability to apply the concept in question, and can make it arbitrary whether a particular application is correct. In the light of this, Lewis' claim (that no correct analysis of CAUSATION can deny its vagueness) should be reinterpreted. We can interpret it as the claim that an analysis of CAUSATION cannot be made more determinate without faulting in other, more vital regards.[65] For instance, he could

theories or supplementary epistemological accounts. For instance, we could not know anything about atoms without theoretical suppositions. Therefore, the problem with the overall similarity ordering is not that we are unable to apprehend it at first glance. If the ordering can be determinately computed with the supplement of an epistemological account, nothing is wrong with it.

[65] Either the claim is that or—if 1973 Lewis believed in prelaid conceptual truths, which is likely—that the concept of over-all similarity between worlds is built into the concept of a counterfactual, and that the concept of a counterfactual is built in to the concept of causation. The aim of this thesis is in part to defeat the idea that the contents of concepts are laid out in *a priori* truths, provided that *a priori* truths

argue that any analysis of CAUSATION that does not apply counterfactuals is bound to run into troubles with separating causes from effects, preemption, and all the other issues that plagued the regularity analysis of CAUSATION (and that any understanding of counterfactuals is bound to rest on similarity comparisons or some equally indeterminate interpretant). Or he could argue that any analysis that is made with more determinate ACs is bound not to capture some of the *de facto* usages we make of CAUSATION; that the pre-analytical concept is indeterminate, and so that the analytical concept must match up with this indeterminacy should it capture the pre-analytical concept. I am not claiming that these arguments are correct, but we must hypothesize that some argument of the kind would have to be correct if Lewis' claim—that indeterminacy is inevitable to causal analyses—is to come out right. Hence, the indeterminacy that infects analyses of CAUSATION is not inevitable *per se*, but inevitable with respect to satisfying other norms we correctly hold the analysis answerable to.

There are three mitigating factors to the indeterminacy of the foregoing analysis. One is what Lewis himself argues (1973a: 559-560): We do have some expectations as to which respects of similarity should weigh more than others, and these are legitimate to lean on in order to achieve more determinate causal judgments (cf. Lewis 1979). As Lewis says, "The prevailing laws of nature are important to the character of a world; so similarities of law are weighty. Weighty, but not sacred." (Lewis 1973a: 560) Also, when judging a particular causal claim, we do not in practice have to consider *all* respects of similarity between worlds. For instance, when judging whether the terrorist attack in Norway caused a boost in community feelings—i.e. judging whether the closest possible world where neither the terrorist attack occurred nor the community feelings were boosted, is closer than a world where the terrorist attack occurred, but no community feelings were boosted—it is unnecessary to consider whether it was raining in Japan at the time of the attack in the candidate worlds. But it is impossible to specify, beforehand and in full generality, all and only

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are conceived to be something else than what a good analysis takes the ACs of a concept to be. Since I take the goodness of an analysis to consist in its satisfaction of many kinds of norms, some of which are substantiated by their practical utility, it is at best odd to equivocate "a good analysis" with "an analysis that expresses an *a priori* truth." Hence, this envisaged avenue of argument is not available to us.

the respects that will be relevant for judging any causal claim. The second mitigating factor is that the pre-analytical concept of CAUSATION is not perfectly determinate (Woodward 2003: 84-93). It is not as if Lewis supplied ACs that are *significantly* less determinate than that concept. But some analyses do.[66] Lewis' analysis does not come out terribly bad with respect to (D). The third mitigating factor is that Lewis' analysis scores well on most other counts. Even if it does not excel with respect to (D), it contributes significantly to conceptual integration and arguably, it reflects ordinary, *de facto* causal claims quite well.[67]

3.1.6 Explanatory and Expressive Utility

The last of the rational and pragmatic norms is built on the idea that philosophers can produce conceptual analyses that in some way benefit our practices of explaining things with the help of the concept in question, and of expressing things with the concept that the ACs of the analysis make clear to us. When philosophers do, the idea goes, they have achieved something praiseworthy.

> **(EEU) Explanatory and Expressive Utility**: A conceptual analysis should at least remain neutral to, and preferably benefit, our practices of employing the analyzed concept to explain phenomena and to express our thoughts, by supplying ACs for the concept that could, in the explanatory or expressive context, be substituted for the application of the concept for gain in explanatory strength or expressive clarity.

It is not always clear where the line goes between explaining something and expressing something. For instance, sometimes we speak of explaining our thoughts, and that typically involves reasoning to and from the thought. This process reveals its significance. Therefore, explaining a thought also works towards explicating the

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[66] Cf. our layman concept of intuition in comparison with Chudnoff's analysis of an "INTUITION EXPERIENCE" in section 2.1.5.

[67] To the latter point we may add that Lewis revised his analysis to accommodate common judgments about cases of redundant causation (Lewis 2000).

content of that thought, i.e. expressing it.[68] This is why explanatory utility and expressive utility have been lumped together in a single norm.

Using a concept to express a thought and using it to explain something are very common discursive activities (i.e. activities in which we employ concepts). Concepts are not just abstract entities. They are quite literally tools for enabling the actions we have an interest in performing as cognizant beings. Of all the things we may have an interest in doing as such, it is clear that giving explanations and expressing our thoughts are among the most important. The importance of these activities to our epistemic lives is what warrants the suggestion that conceptual analyses ought to contribute to the possibility of their execution. It would be completely unreasonable to hold a norm of this kind if it were for utility in some narrow kind of discursive activity, such as solving algorithms. Not all concepts are used for solving algorithms. By contrast, it does seem that all of our concepts are employed in expressing thoughts and explaining the things to which the concepts apply.[69]

Even so, it might strike some as wrong to demand that conceptual analyses somehow contribute to these activities. We might agree that benefitting explanatory and expressive practices is a good thing, but the contested issue is whether it is a proper demand on conceptual analysis. It could be argued that it is the task of those who employ the concept to express themselves in clear ways and to make good explanations with the concept: Analysts have no responsibility for the well-being of these activities. All they are responsible for is to explicate the content of the concept, however well-suited that content may be for expressive and explanatory purposes. I have two responses to this kind of argument. The first is that "those who employ the concept" includes analysts, and this has significance for what norms analyses are answerable to. The next chapter argues that we participate in the practice of employing the concepts we analyze, and therefore, that the concerns practitioners have with the concept are also our concerns as analysts. The second response is that,

[68] For inferentialists, such as Brandom, the inferences a contentful item is caught up in are what make that item contentful in the first place. We need not assume inferentialism to make the point in the main text, although it is more obvious from that point of view.

[69] It does not at all seem possible to express thoughts without employing concepts. As Margolis and Laurence say in the first sentence of their encyclopedia entry on "Concepts": "Concepts are the constituents of thoughts." (Margolis and Laurence 2011)

without any practical concern of the kind expressed by (EEU), philosophy would not seem worthwhile doing. Our concerns do not have to be as directly practical as (EEU) recommends, but in the end, there should be some value in doing philosophy, beyond satisfying "disinterested intellectual curiosity." My remaining purposes in the present section are to comment on the relation between (EEU) and the other norms, and to prove that it is possible for a conceptual analysis to satisfy (EEU). The latter might not be obvious.

(EEU) is not independent of the other norms. In particular, it is intimately connected with the norm that conceptual analyses ought to advance our understanding of the analyzed concept, and the norm that an analysis ought to provide determinate ACs. Our explanations are better when the obtainment of the *explanans* and *explanandum* are more determinate matters. Operationalizing a concept is making it fit for categorizing data, and ascribing determinate ACs to the concept is an inevitable part of any operationalization. Moreover, when an analysis provides ACs that are better understood than the concept is pre-analytically, we benefit our practice of using that concept to express thoughts.

Even if (EEU) is intimately connected to the other norms, it has a different perspective. Consciously obeying it has us focus on the specific tasks for which we intend to employ the concept. While (D) and (CI) only relate indirectly to the utility of employing the concept, (EEU) relates directly to what particular tasks we have the concept for. In that way, there can be context sensitive concerns in play that decide whether (EEU) is satisfied in a given case. It will depend on what we want to express and explain. For instance, in justifying my conception of intuitions, I claimed we should favor the account I gave over the account that claimed that intuitions are inclinations to believe. The account I gave, but not the latter, made sense of the sort of dialectical role we would like to be able to comment on when expressing our tentative commitments. For philosophical argument in particular, it is more useful to have a concept that expresses the dialectical role I claimed for INTUITION, than a concept that does no more than express how the interlocutor relates to the intuited content as something she is prone to believe. In this way, the account could be justified for its satisfaction of (EEU) with respect to a specific context with specific expressive needs. Note that we do not even have to ask whether this dialectical role is contained in our

pre-analytical concept of intuition to see that it is a valuable feature of the account. There are not always such context specific expressive needs in play, but when there are, it is better if the analysis meets those needs.

Woodward's (2003) analysis of CAUSATION is a great example of how an analysis can satisfy (EEU). While the case can be made that it, too, meets specific, contextually present needs—it provides an operational framework utilizable for present-day science; how causal relations can be uncovered by *experiments* is made explicit—it is a much more general purpose of ours to give causal explanations, than to comment on the dialectical role of our tentative commitment in the context of a philosophical argument. Woodward is highly aware of the utility he aims at providing. He writes the following under the heading "Motivation":

> What is the point of having a notion of causation (as opposed to, say, a notion of correlation) at all? What role or function does this concept play in our lives? An important part of the appeal of a manipulability account of causation is that it provides a more straightforward and plausible answer to this question than its competitors. (Woodward 2003: 28)

It would be modest to take this only as a *motivation* for developing the kind of account Woodward does. According to the inclusion of (EEU) as a norm for conceptual analysis, the practical, explanatory utility of Woodward's account also counts towards its justification.

How does Woodward's account satisfy (EEU)? The answer to this question should show that it is possible that it could not have satisfied (EEU) while scoring no worse on how well it reflects our pre-analytical concept of causation. It should not satisfy (EEU) just because it happens to be an analysis of CAUSATION. Causal explanation is a primary kind of explanation. If any analysis of CAUSATION would count as satisfying (EEU) in virtue of that fact, (EEU) would not be very interesting. But this is not the case. There are specific features of Woodward's analysis that makes it utilizable as a theoretical foundation for giving causal explanations.

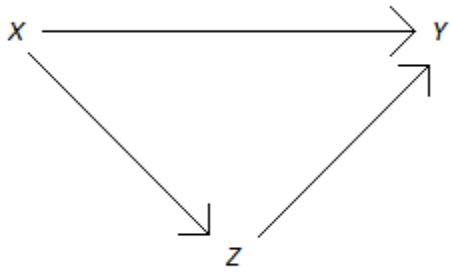
In section 3.1.4, I stated that Woodward analyzes CAUSATION in terms of the changes an actual or hypothetical intervention on a candidate cause would make to its candidate effect. Its satisfaction of (EEU) is not merely a heuristic matter of using vocabulary that directly reflects an experimental context. We have to flesh out the

analysis to see why it counts as satisfying (EEU). In Woodward's set-up, the causal relata are not just events or states that can either occur or fail to occur, obtain or not obtain. Instead, *variables* are recruited to represent the relata of a causal relationship (Woodward 2003: 38-39). These can take two values (e.g. {*occurs*, *does not occur*}), but they can also take many values, depending on exactly how we should and are able to represent the candidate causes and effects in question. Already by this move, the analysis contributes to explanatory utility. We are not always satisfied with explaining the fact that some event occurred. Sometimes we want to explain why it occurred in the way that it occurred, and an analysis of CAUSATION should support our opportunity to do so. For instance, if the events in question are shootings of billiard balls—in which the first ball collides with the second to send it off in some direction— we might want to explain why the second ball shot off with the speed it did shoot off with. To that end, Woodward's analysis tells us that we should employ a many-valued variable *S* to represent the shooting of the second ball, and let its values $\{s_1, s_2, \ldots s_n\}$ correspond to the speed the ball moves in. Obviously, there are many speeds, not just *speed* and *not-speed*. The speed of a moving object is a quantifiable, measurable property, so many-valued variables are perfect instruments to represent movements if we are interested in explaining their speed.

Furthermore, many-valued variables can be employed even when the values of the relevant variables do not correspond to quantified degrees of some sort. Consider Woodward's treatment (2003: 57-58) of the following example, originally due to McDermott (1995: 531). A dog bites off McDermott's right forefinger. The next day he goes on to push a button with his left forefinger. This causes a bomb to explode. Now, the dog bite causes McDermott to push the button with his left forefinger, since he would have used his right forefinger had it not been bitten off. How do we avoid the verdict that the dog bite causes the bomb to explode? Woodward's solution is elegant, and reinforces our belief in the explanatory and expressive benefit from using many-valued variables. The solution is to treat the pushing of the button as a three-or-more-valued variable {*pushes with left forefinger*, *pushes with right forefinger*, *does not push*}. Only the first two of these counterfactually depend on the relevant values of the dog bite {*occurs*, *does not occur*}. As might be obvious, changing the value from *pushes with right forefinger* to *pushes with left forefinger* does not change

whether the explosion occurs. Hence, no change in the value of the dog bite-variable changes whether the explosion occurs.

Causal networks can be complex. There is more to say about them than facts of the sort *A* causes *B*. We might want to know more specific facts about the relation between *A* and *B*. We might want to know what *function* connects the values of *A* with the values of *B*. Woodward has several examples of how it can be crucial to know exactly what function that is, and what values it connects. For instance, there is an example where *X* causes *Z*, and *Z* causes *Y*, but where in addition, *X* causes *Y* independently of the path through *Z* (Woodward 2003: 49):



In this case, if we say nothing about the functions that link the variables, we would assume that intervening to change the value of *X* would change the value of *Y* (after all, the effect of *X* on *Y* would seem to be doubly positive). However, there are functions for this sort of structure by which intervening to change *X* would not change *Y*. If the functions are such that the direct path from *X* to *Y* exactly cancels out the effect from the other, indirect path via *Z*, or vice versa, then no intervention on *X* alone would change the value of *Y*. For instance, say *Z* has a negative impact on *Y*, and the direct effect of *X* on *Y* is positive. Then, pending *how* negative and *how* positive these impacts are (which is what the functions should make clear), we cannot say that *X* causes *Y* without qualification. If the effects cancel each other exactly out, we cannot, according to Woodward, say that *X* is a "total cause" of *Y*, even if it is, in another sense, a "direct cause" of *Y* (Woodward 2003: 51, 55).

The distinctions between DIRECT CAUSE and TOTAL CAUSE (and also: CONTRIBUTING CAUSE) are important components of Woodward's analysis. In fact, Woodward gives several analys*es* of CAUSATION. The minimal analysis consists in the necessary and sufficient conditions for CONTRIBUTING CAUSE (Woodward 2003: 59). DIRECT CAUSE

and TOTAL CAUSE are more specific concepts of causation, explicated by adding conditions to the minimal analysis (Woodward 2003: 51, 59). This multitude allows us to express well-defined dissimilarities between how the cause and effect of a causal relationship are related. It enables us to represent complex causal systems by having some variables relate directly to others, and indirectly to yet others.

With Woodward's account, it is also apparent how we should go forward to assess causal claims. To test a causal claim, we must execute interventions or combinations of interventions on the variables in the causal system. After setting its preceding variable(s) at specific values, we observe what value the effect variable attains. By contrast with Lewis' analysis, handling causal claims is a tractable affair with Woodward's analysis in hand. We do not have to try and weight how similar possible worlds are to the actual world. Instead, we can literally realize the antecedents of the relevant counterfactuals, and check the outcome. Sometimes, however, it is not physically or practically possible to carry out interventions. In those circumstances we must *hypothesize* what would happen under the proper interventions. It might not fare better than Lewis' analysis in those circumstances. It still gives us better practical guidance for how we should go about to test a causal claim in the circumstances that do allow for interventions. Moreover, it is advantageous that it does provide that guidance.

There is not space to specify every respect by which Woodward's analysis satisfies (EEU). The foregoing features should serve to show that it is indeed possible for an analysis to contribute to fulfilling the expressive and explanatory purposes we have with employing the analyzed concept. The fulfilment of these purposes is not something that Woodward's theory succeeds in doing simply because it happens to be an analysis of the concept CAUSATION, and not some other concept that is not as important for constructing explanations. Most other analyses of CAUSATION do not succeed in providing explanatory and expressive utility in the ways we have seen Woodward's account do. Hence, its satisfying (EEU) is not just a matter of capturing our pre-analytical concept of causation. It is a matter of designing ACs that we can, for benefit, check with to explain and express the things we have an interest in explaining and expressing. Employing our concept of causation without the guidance

of these ACs (or ones similar in their satisfaction of (EEU)) is less likely to succeed with respect to our explanatory and expressive purposes.

3.2 The empirical norm

The previous norms have all been justified with reasons having to do with how it is favorable to have our concepts shaped and ordered. The dimensions of correctness having to do with what particular contents the analyzed concepts *actually have*, have been bracketed. If we were to rely solely on the foregoing norms for judging whether an analysis is good, we would not be able to see that its goodness has anything to do with whether it correctly represents or reflects the analyzed concept. It has not been argued that following these two guidelines individually—1) producing an analysis in agreement with the rational and pragmatic norms, 2) producing an analysis that matches up with the pre-analytical concept—will inevitably result in different analyses. However, nothing in the argument so far has *guaranteed* that the pre-analytical contents of concepts will be reflected in the analyses they are given. On premises of the foregoing norms only, there is no reason why an analysis of JUSTICE should not have the ACs that are normally associated with the concept of knowledge.

It seems that something crucial has gone missing. To fill in this hole, we need a norm that prohibits such analyses as the one imagined, where JUSTICE is analyzed as if it were KNOWLEDGE. Analyses must somehow reflect the pre-analytical concept that is being analyzed. Without further ado:

> **Conservation (C)**: The ACs of a conceptual analysis should obtain for as many instances as possible to which we would, pre-analytically, apply the concept.

If an analysis satisfies (C), we can say that it conserves the pre-analytical content of the analyzed concept. I think this norm states, in a somewhat unusual way, a feature that is more often held as the primary and distinguishing norm for conceptual analysis, particularly from the viewpoint of the debate about the role of intuitions (cf. footnote 6). Recall Gutting's demarcation of analytic philosophy: It is a thought tradition that aims at explicating our pre-theoretical commitments (Gutting 1998: 10). (C) is stated in precisely that spirit. Our employment of the pre-analytical concept can be considered a commitment of ours—to the effect that we need a concept with that

content—and this norm constrains analyses by having them conserve that content. How we choose to employ that concept can also be considered commitments of ours to the effect that these employments of the concept are in accordance with its content.

The discussion of this norm is divided into three parts. First, I discuss how the content of a concept is given independently of an analysis—what kind of facts decide what the content of the pre-analytical concept is? Second, I argue why we should not require a perfect match with the pre-analytical concept. Third, I argue why we should at all hold this norm if we already hold that conceptual analyses are normatively constrained by the rational and pragmatic norms.

To the first point, then, we should ask: How is it that concepts already have a content that an analysis can either capture or fail to capture? Clearly, the concepts we analyze are already contentful. The question is how we can access that pre-analytical content to see how well an analysis matches up with it. In the absence of better options, let us assume that concepts attain and maintain their content by being applied in practice, through the use of expressions that are associated with the concepts.[70] Therefore, the pre-analytical content of a concept is somehow revealed in how its users in fact apply the concept in question, and how they would apply the concept when they confront hypothetical scenarios. However, we cannot *identify* the pre-analytical content of a concept with any simple or complex function of its factual employment. It is just not true that facts about the contents of concepts can be reduced to facts about how those concepts are in fact applied. This is a (if not *the*) core lesson from Wittgenstein's rule-following arguments (Wittgenstein 1997).[71] The problem with reducing facts about the contents of concepts to facts about how users of those concepts in fact apply them, is that it is impossible to bridge the gap between *regular* performance and *correct*. As Brandom notes, "The problem is that any particular set of performances exhibits

[70] Other options include holding that concepts have their content thanks to their place in a Platonic order of ideas, or holding that we are each born with a "language of thought" in which the contents of our concepts are inscribed into the rules of that language. I dismiss these options since they postulate epistemically inaccessible entities and structures. The fact that we do assess, with significant success, how well analyses capture the contents of the analyzed concepts speaks against the plausibility of these alternative views.

[71] At any rate, it is a core lesson of those arguments as they have been expounded by Kripke (1982: 25-37) and Brandom (1994: 26-30).

many regularities. […] For anything one might go on to do, there is some regularity with respect to which it counts as 'going on in the same way,' continuing the previous pattern." (1994: 28). Unless we already assume the distinction we are trying to make intelligible—between correct and incorrect applications of a concept—there is no way to pick out the right respect of regularity for which the content of a concept is given. There is no way to block off incorrect applications of a concept if our only means to pick out the right set of performances are purely descriptive, such as by regularities or dispositions.

This is indeed problematic. The means that I have suggested for ensuring that an analysis matches up with the pre-analytical concept are dispositional in kind. (C) states that the ACs of an analysis should obtain for an extent of instances that a person who possesses the concept *would*, pre-analytically, apply it to. However, we should not take this problem as undermining any claim we have to knowing something about the pre-analytical content of a concept. What the problem does show is that it is useless to collect evidence about how concepts are in fact employed (or intuitions towards employments) if the hope is to determine what the pre-analytical content of a concept is. We cannot treat such facts as undeniable data to that effect. There are no ultimate and indisputable facts we can appeal to, to settle what the pre-analytical content of a concept is. (The epistemology of conceptual analysis is controversial even in the event that we take it to be a purely conservative enterprise.) Still, we need some indication of what the pre-analytical content of a concept is. To this end, we have no choice but to do such things as consider hypothetical scenarios and ponder whether we would apply the pre-analytical concept to the candidate instance. The problem is not as harmful as it might seem. While we cannot reduce facts about the contents of concepts to facts about how they are or would be applied, we are ourselves in the position of being users of the concepts we analyze. Hence, even as analysts (or better: *particularly* as analysts), we are in a position to distinguish correct and incorrect applications of the analyzed concept. This holds even when the applicability is decided pre-analytically, and not on the analysis' premises.

Moreover, this problem disturbs *any* account of the methodology of conceptual analysis. In fact, it is less of a problem for the account I am defending, seeing as it does not identify how good an analysis is with how perfectly it captures the pre-

analytical concept. Even if it is necessary to factorize how well an analysis captures the pre-analytical content of the analyzed concept, this is not its be-all and end-all. This brings us to the second point of the discussion of this norm, which is why it is wrong to require that an analysis capture the pre-analytical concept perfectly.

The reason why we should not require that the analytical concept has to match up perfectly with the pre-analytical concept is that there is no guarantee that the pre-analytical concept is not defect in some way or another. For instance, it might be less determinate than what we would prefer our concepts to be. Or the content of a pre-analytical concept might license incompatible applications. In that case we need to revise the concept, or construe two different concepts to do the same expressive, representational, and explanatory work as the original concept was set to do. A case can be made that our pre-analytical concept of a belief is defect in the latter way. Specifically, the argument would go, there are too big differences between the sense in which one has whatever belief one is willing to acknowledge, and the sense in which one has whatever beliefs one ought to have, given what else one acknowledges. In the first sense, the extension of BELIEF is circumscribed to the contents one would avow to, but none beyond that. In the second sense, it is circumscribed to what one would avow to, in addition to the consequences of those contents. These senses are both contained in our pre-analytical concept of a belief. To illustrate, someone might believe that the shape of the earth is approximately like a ball. But if he or she is without mathematical training, our pre-analytical concept of belief does not, in this case, tell us whether we should affirm or deny that the person believes that the earth has the kind of shape whose volume is approximately determined by $4 \times \pi \times$ *the radius of the earth*$^3 \div 3$.

Brandom (1994: 195) takes reasons such as these to avoid employing BELIEF. Instead, he goes with COMMITMENT to do the associated expressive and explanatory work. However, we do not *have* to resort to a different concept to clarify what we mean in the cases where we would, pre-analytically, apply BELIEF. We might instead want to revise our concept of belief to prevent it from delivering contradictory verdicts. But that would have the inevitable consequence of breaking off with the pre-analytical concept of belief. Since we do not want a concept that allows for contradictory verdicts, this would be a legitimate kind of revision. Hence, if this is generalizable (as

we have no reason to doubt), conceptual analyses do not have to capture the pre-analytical contents of concepts perfectly. This claim gains plausibility from the independently plausible idea that our conceptual scheme has evolved for the better through the course of history, and, while we have come far in our collective intellectual development, we need not assume that our current state is the final state.

The Wittgensteinian argument presented above—to show that the content of a concept cannot be identified with a function of its factual employment—rests on the premise that not all tentative applications of a concept are correct with respect to the content of that concept. The premise for my claim in this point of discussion—to show that an analysis does not have to capture the pre-analytical concept perfectly—is at another level. Here, there is not a fault at the level of the applications that are being made of the pre-analytical concept. Rather, it is the possibility of a fault in the pre-analytical concept itself that makes it wrong to require that an analysis has to construe the analyzed concept in a way that has it (the analytical concept) match up perfectly with the former (the pre-analytical concept). If we accept that the match should not be perfect in all cases, however, we should wonder what legitimacy, if any, (C) has. This brings us to the third and final point in the discussion of this norm.

From the perspective of the rational and pragmatic norms, there is no apparent reason why we should conserve the contents of our pre-analytical concepts. Why should we restrict ourselves to *revise* our conceptual scheme when, patently, we have the opportunity to *revolutionize* it for optimal utility? There are at least two reasons why we should hold (C), despite the legitimacy of possible mismatch with the pre-analytical concept. The first is that radically changing the contents of our concepts jettisons our capability to understand each other. We need to conserve the contents of our concepts, or else we run the risk of not being able to communicate.[72] This point is purely practical, but it has no less force for that reason. The motivation for licensing radical revision is itself substantiated by practical concerns. Thus, the claim that we should radically revise our conceptual scheme for optimal utility faces self-defeat.

[72] We could also argue that it is *impossible* to revolutionize our conceptual scheme, that the scheme could not have been radically different (cf. footnote 49). After all, in revising the content of a concept by giving it a revisionary analysis, we have no choice but to use other concepts from our conceptual scheme in the ACs. In that way, we have to use our conceptual scheme to change it. That makes erasing that scheme and putting a new one in its place an impossibility. Cf. Davidson (1984).

The second reason to conserve the pre-analytical contents of our concepts parallels one of the reasons given above, for the contrasting claim that we should not preclude revisionary analyses in principle. The reason given there was that, despite the plausibility of the idea that our conceptual scheme has evolved for the better through our collective intellectual history, we should not assume that the current state of our conceptual scheme is the optimal state. What I now claim is that, despite the wrongness of assuming that all our pre-analytical concepts are in perfect shape, we should be careful in entertaining the idea that our pre-analytical concepts are not just how they should be. It would be foolish to assume that indefinitely many years with coping with the world (and coping with our coping with the world) has not furnished us with pretty useful concepts. That is, it is *prima facie* plausible that there is something wrong with the analysis, and not something wrong with the analyzed concept, if it is the case that a concept is pre-analytically applicable in some circumstance, but the necessary and sufficient ACs of that analysis do not obtain. Naturally, this sort of issue has to be decided on a case by case basis.

So we can require that conceptual analyses conserve the contents of our pre-analytical concepts, as long as we do not take the failure of an analysis to do so for a particular instance as conclusive evidence of its failure. I think we are quite able to handle this nuance. Moreover, a conceptual analysis can be good even if it fails to satisfy (C) to a desirable degree. Just as with the other norms, there is a trade-off between failing on one count and succeeding on another (cf. section 4.1). It would not be possible to draw these conclusions if we held that the pre-analytical content of a concept is laid out in unrevisable (*a priori*) truths, and that the only proper goal for a conceptual analysis is to capture those truths. The next chapter elaborates on how the present account of conceptual analyses does not let TRUTH do all the work.

3.2.1 Objection

"In the previous chapter, you argued against the evidential status of intuitions. But in order to satisfy (C), we have to use intuitions as evidence, since they are the only available means we have for assessing the pre-analytical contents of concepts. It does not help that (C) is one among several norms."

The objector is right in claiming that intuitions were denied evidential status. But the objection ignores the other side of the same coin, that intuitions are not in general

blameworthy. Every intuition is liable to challenge, but that does not imply that no intuitions are correct (i.e. that no intuitions employ the target pre-analytical concept correctly). We are entitled to make a claim without justifying it as long as there is no proper challenge to it on the table. At some level or another, we have to simply apply concepts without justifying our application (or else regress obtains, and we cannot claim anything). Following the Wittgensteinian contention that there are no descriptive facts that alone determine what the content of a concept is, there may be no non-controversial starting points to give as premises for showing why some analysis satisfies (C). In that case, expressing a claim as an intuition is just being honest about the liability-to-challenge that claim stands under. We cannot know, in advance, all the challenges that may be leveled against the claim, and so we need not know how to justify it. As users of the pre-analytical concept, we are fully entitled to make intuitional claims with that concept, until properly challenged. Thus, we are entitled to use these claims as elucidating the pre-analytical content of a concept, despite the fact that they are not, strictly speaking, *evidence* for what that content is.

4. Mastering Concepts

The previous chapter discussed the norms in isolation from each other. I only gave individual reasons for holding them. This chapter assumes a broader perspective. First, I discuss how the norms relate to each other, and how they function as conditions on being a good analysis. Second, I argue that *good analysis* is not equivalent with *true analysis*. Conceptual analyses are set apart from other claims by how they are properly justified. In justifying a conceptual analysis, we ought to treat it (partly) as purporting to determine the content of the analyzed concept. In so treating the analysis, its truth value is irrelevant. Hence, the traditional notion of a conceptual truth is discarded in favor of the notion of a good analysis. Third, I argue that the rational and pragmatic norms have their grip on us because we are participants of the practice of employing the concepts we analyze. Our purposes as concept-users substantiate the rational and pragmatic norms. I argue that the perspective we thereby assume is reasonable and philosophically worthwhile, and contrast it with a psychological perspective on concepts.

4.1 The relations between the norms and to the goodness of analysis

The empirical norm (C) is clearly different from the rational and pragmatic norms I have stated. This remains true even if holding (C) is itself rational. In the concluding part of this section, we shall see how distinct (C) really is. Things are fuzzier with the interrelations between the other norms. For some pairs of norms—such as (U) and (CI); (D) and (EEU)—there are clearly relations of dependency and redundancy. For instance, in a given case, there might not be more in satisfying (EEU) than to satisfy (D). Notwithstanding the relations of dependency and redundancy I have noted in passing, I have not mapped out all the relations between the norms. This is an undeveloped field, and needs some place to start. I have found these norms worthy of defense, and have put them forward as such. The hope is that they can serve as starting points for further development.

The norms relate collectively to the goodness of analysis. Thus, if an analysis satisfies all the norms of the previous chapter, it is a good analysis. But it does not seem right to claim that an analysis is good *only* if it satisfies *all* the norms. Had that been the case, it is unlikely that there would have been a single good analysis in the history of philosophy; but that seems wrong on independent grounds (despite the dizzying

amount of disagreement in philosophy). That no analyses enjoy universal approval does not prove that no analyses are good. Being controversial is not the same thing as being bad. In fact, we should expect that there is a lot of disagreement in philosophy if it is true, as I have claimed, that there are *multiple* factors that determine whether an analysis is good.

Given that an analysis does not have to satisfy all the norms to be good, it would be preferable if we had something determinate to say about how much *total norm satisfaction* is needed. But it is hard to meet this requirement, for several reasons. First, tallying the number of norms an analysis satisfies—and using that number as basis for deciding whether an analysis is good—makes no sense when the norms are satisfied in degrees, and not in the manner of all or nothing. Second, when these degrees are neither quantified nor—perhaps—quantifiable, it is futile to try to specify a more complex function that connects norm satisfaction to the goodness of analysis. Factor in that there are several legitimate purposes behind conceptual analyses, that these purposes lend themselves to weighting the norms differently, and we see that we are even further away from meeting the requirement.[73] Hence, we cannot, in full generality and with perfect precision, separate good analyses from bad analyses on premises of the present account alone. The less determinate claim we are left with is that satisfying the norms makes an analysis good.

We can think of the norms as expressing argumentative tools we do and should use. We do find circular analyses problematic, and we should continue to do so. The features of goodness associated with the norms in the previous chapter have not received due methodological appreciation, and they have been completely neglected in the debate about intuitions. Even if the present statement of norms does not complete the task of articulating a good conceptual-analytic method, it has been purposeful in the light of the absence of a methodology that acknowledges the rational and pragmatic dimensions of conceptual analysis.

There should be a trade-off between the norms. If an analysis excels on multiple counts, its failure on another count is less blameworthy. However, some norm violations are worse than others. It is worse if an analysis has incompatible ACs than

[73] Below, I elucidate how different purposes can lead us to weight the norms differently.

if its ACs are less than desirably determinate. Only (IC) and (EC) are stated with the "must"-modifier. Only they are intended to be inviolable.[74] The rest of the norms are stated with "shoulds," and should be interpreted as such. In addition, as I suggested in the previous paragraph, there are several purposes embodied in the practice of conceptual analysis. This is seen clearly when we consider a concept with multiple good analyses, for which none of the analyses can claim the status of being *the* correct analysis of that concept.[75] The goodness distinctive of one is associated with its higher satisfaction of some norm. We are not always in a position where we have to choose between analyses, but when we are, the choice can be made with respect to the purpose we want the analysis for—that is, which norms we prioritize the satisfaction of. For experimental scientists, for instance, it is important that concepts are operationalized, so they are likely to prioritize satisfaction of (D) over satisfaction of (CI). Hence, if they are in need of a concept of causation, they would be more prone to appropriate Woodward's analysis than Lewis' analysis. By contrast, should someone find it important to have a conception of causation that is not circular in any way, she would more likely prefer Lewis' analysis by prioritizing satisfaction of (CI). Such prioritizing is perfectly legitimate. There are no *a priori* constraints that stop us from using conceptual analyses to meet our practical, discursive needs. Contrary to the traditional picture that there has never been a successful conceptual analysis, the account I have presented suggests that many analyses are good, even if there is no analysis that is perfect in every way. In particular, it is too demanding to require perfect satisfaction of (C).

Finally, there is a more radical case of norm prioritizing that constitutes an exception to several claims I have made in this thesis. In the last paragraph, for instance, I claimed that (IC) is an inviolable norm, but that claim is conditional on there being no purpose behind the candidate analysis that lends itself to a certain radical

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[74] As for many claims in the previous chapter (cf. footnote 41), there is an exception to this that I discuss below.

[75] Note that different analyses count as being *of* the same concept only insofar as we individuate concepts by their associated expressions, which is the individuation criterion I have been assuming in this thesis. If we individuate concepts by their ACs, it does not make sense to consider different analyses to be of the same concept; they would only be presenting different concepts. There is no consensus on how concepts ought to be individuated. But as long as we clarify what criterion we are relying on, the issue is harmless.

prioritization. The prioritization in question is one where (C) is satisfied at any cost. In a limited number of cases, it is not fruitful to constrain an analysis in rational and pragmatic ways. Sometimes, we want to display the content a concept has attained pre-analytically, with no concern to display that content as if it were rationally and pragmatically shaped. Concepts *can* be defect. They can, for instance, attain contents that license incompatible applications, or contents that are incompatible with true scientific theories. For example, some might want to argue that the concepts SOUL and FREE WILL have contents that are incompatible with prevailing science.[76] If the pre-analytical content of a concept is *too far* from being rationally utilizable, there might be no point in trying to save that concept by giving it an analysis that is rationally and pragmatically constrained. In such a case, it can be worthwhile to prioritize the satisfaction of (C) over all the rational and pragmatic norms, even those with the "must"-modifier. The rational and pragmatic norms lose their force once we have no intention of continuing to employ the concept. Hence, the existence of such examples cannot be used as an objection to the legitimacy of the rational and pragmatic norms with respect to the far more usual case where we do intend to employ the concept. For all the individual reasons I gave for holding the rational and pragmatic norms, we *should*, in that more usual case, have the ACs we supply a concept be constrained by the rational and pragmatic norms.

There is a thin line between analysis and synthesis. In case a concept has a pre-analytical content that is rationally non-optimal, we can choose to disregard some part of its content by giving the concept a revisionary analysis. Clearly, this move is somewhat synthetic, but if it conserves a large part of the pre-analytical content, it does not seem synthetic enough to deny it the label "analysis." Alternatively, we can choose to remain completely true to the pre-analytical content by prioritizing satisfaction of (C) at the cost of dissatisfying the rational and pragmatic norms. Then we get a more purely analytic analysis. The respective proprieties of these lines of attack depend on whether we intend to continue employing the analyzed concept. Both can count as good analyses, pending different purposes. The important thing to note is that the propriety of the latter line of attack—where we display the pre-

[76] Other examples include pejorative concepts, such as BOCHE and NIGGER, which, according to Brandom, incorporate bad inferences from their "circumstances of application" to their "consequences of application" (Brandom 1994: 125-130).

analytical content of a concept despite its flaws—is only worthwhile in the context of acknowledging that there are practical purposes behind our concepts, such that, if we were to have a purpose of employing the analyzed concept, we could not be committed to the cast of it suggested by the analysis. The possibility of good analyses that are not constrained by the rational and pragmatic norms hinges on the fact that they serve rational and pragmatic purposes, by displaying the irrationality or impracticality of a pre-analytical concept. More usually, however, we are better off explicating the contents of our concepts in a charitable manner, by constraining analyses by the rational and pragmatic norms. The strategy of producing an analysis in complete disobedience of those norms is only optional when the pre-analytical content is severely flawed.

4.2 Two features of the justification of conceptual analyses

If we do not distinguish conceptual analyses from ordinary bi-conditional claims, we are not able to see that they should be judged by anything other than their truth value. As I shall argue in this section, we should rather hold conceptual analyses subject to a more complex kind of goodness. First we have to consider a plausible way of justifying conceptual analyses, had they merely been subject to truth.

Treating an analysis as an ordinary bi-conditional claim, with a truth value we aim to settle, goes hand in hand with an intuition-centered method. If there is no more to judging them than settling their truth value, then there is no more to their justification than showing, to whatever extent possible, that there are no counterexamples to them. That is, there must be no case where the ACs obtain but the concept is not applicable, and no case where the concept is applicable but the ACs do not obtain. If there are such examples, the analysis is false. Thus, a natural way to figure out if we should hold an analysis, on this picture, is to use imaginative exercises. The relevant exercises consist in constructing hypothetical scenarios where, intuitively, the concept is applicable, but the ACs do not hold, or vice versa. If these exercises do not result in a counterexample, we are justified in holding the analysis. If someone comes up with a counterexample, our justification for holding the analysis is defeated, unless we can show that the counterexample is ineffective.

The introduction of rational and pragmatic constraints comes as an alien addition. These constraints have nothing to do with the truth value of the analysis. Therefore, it

would seem that their introduction makes it even more difficult to achieve an analysis worthy of holding: Not only must the analysis be true, it also has to satisfy a number of norms relating to its practical and rational worth. Plausibly, there has been no analysis in the history of philosophy without a counterexample. Thus, it is yet more plausible that there has been no analysis in the history of philosophy that satisfies all the rational and pragmatic norms *in addition* to having no counterexample. However, my strategy has not been to lump the rational and pragmatic norms together with the norm that there must be no counterexample to the analysis, and consider all of them to be necessary conditions on the goodness of analysis. While both (C) and the group of rational and pragmatic norms have been considered as determining the goodness of an analysis, the demands associated with them have been leveled down.

An analysis does not have to satisfy all the rational and pragmatic norms to be good, and neither does it have to be without a counterexample. Weakening the latter demand is the most conspicuous move. It involves holding that counterexamples to an analysis do not necessarily defeat it. What counterexamples do show, on *any* account, is that the analysis does not capture the pre-analytical content of the concept with respect to the given case. On a more traditional conception of conceptual analysis, this is equivalent with the falsity of the analysis, and hence its defeat. But what if we do not require a perfect match with the pre-analytical concept? Once we do not hold that counterexamples straightforwardly defeat an analysis, it is irrelevant to "check" its truth value. Once we hold that, we are treating the analysis as determining the content of the analyzed concept, and so, the analysis will be vacuously true. It will, so to speak, stipulate its own truth.[77] Now, allowing claims to stipulate their own truth is an awful way of treating them if we do not have other, non-truth-relating norms to judge them by. If we had none, it would be arbitrary whether we should accept them. Since we do have non-truth-relating norms to judge analyses by, it is not arbitrary whether we should hold an analysis when its truth does not matter.

[77] There is another option. If we continue judging the truth value of an analysis according to the pre-analytical content of the analyzed concept, the analysis will be false if there is a counterexample to it. But its falsity would not be paramount to its defeat, so the practical outcome of favoring this alternative stays the same.

We can extract and juxtapose two dimensions of an analysis' goodness from this dialectic. On the one hand, an analysis is good insofar as it applies the analyzed concept in accordance with the pre-analytical content of the analyzed concept. Thus, on the one hand, we are treating the analysis as with a predetermined content. On the other hand, we can treat an analysis as good insofar as it purports to determine the content of the analyzed concept in a manner that satisfies norms for good concept formation. If we treat an analysis this way, it is fruitless to assess its truth value. If a claim is interpreted as determining the content of one of its constituents, the truth value we should ascribe to it is determined by the claim itself. Hence, there must be other norms that structure this second dimension of goodness. The articulation of the rational and pragmatic norms is my attempt to spell out this second dimension. In defending them, I based my arguments on how concepts ought to be formed. For instance, in defending the norm for conceptual integration (section 3.1.4) I argued that, since concepts ought to unify, conceptual analyses ought to contribute to conceptual unification. I extended the argument for having concepts in the first place to show that analyses answer to the same demand. The success of extending such an argument requires that a conceptual analysis determines, or purports to determine, the content of the concept in question.[78]

These seem to be incompatible ways of treating an analysis. We cannot treat the content of the analyzed concept as already determined if we are treating the analysis as purporting to determine the content of that concept. It would seem that we have to choose between these manners of treatment.

The trick I have done is to assimilate the first way of treating an analysis to the second kind. The rough theory I have presented of how we ought to justify conceptual analyses *does not* warrant treating an analysis as an ordinary bi-conditional claim, whose primary value designator is its truth value. Despite not recommending this treatment of analyses, the theory I have proposed has not been in disregard to the fact that the concepts we analyze are already being employed by practicioners in a

[78] Someone in want of a purely analytic conception of conceptual analysis is surely disappointed by this. Considering analyses to purport to determine the content of the analyzed concept is introducing a synthetic, normative element to a conception of conceptual analysis. Maintaining (C) as a norm is my justification for still calling this "conceptual *analysis*."

discursive practice. That is, the theory has not conceived of analyses as attributing contents to concepts from scratch. Instead, the fact that the concepts we analyze already have a content—a content they presumably have attained in practice—has been paid respect by demanding that analyses to a large extent conserve that content.

The assimilation has consisted in translating a presupposition of the first way of treating an analysis—that the contents in question are already determined—to a norm for the second way of treating it—that the content a concept already has attained ought to be conserved in re-determining that content. If we had stuck to just the first way, we would have to deem it impossible for an analysis to transgress the boundaries of the pre-analytical concept. That would make the analysis false. By taking analyses to be content determining *as well as* content determined (each to limited extents), we have been able to allow that the analytical concept does not have to match up *perfectly* with the pre-analytical concept. An interesting consequence of this weakened requirement is that the truth value of an analysis no longer decides whether we should hold it. Hence, we discard the traditional notion of conceptual truth. We replace it by the notion of a good analysis, and take this notion to incorporate a demand that the analysis to some extent conserve the content that the analyzed concept already is imbued with.

Following the arguments of Quine (1951) and Williamson (2007), the notion of a conceptual truth is highly problematic. If it is sensible to speak of conceptual truths on the account I have presented, they are not vulnerable to the relevant objections.[79] The account does not sharply distinguish between analyticity and syntheticity. Because there are synthetic features of the account's notion of good analysis, the account does not presume that there are necessarily any interesting analytic/conceptual truths contained in our pre-analytical conceptual scheme, waiting to be discovered.

4.3 Being a participant of a discursive practice

In an epoch-defining essay for Norwegian academia, Hans Skjervheim distinguishes the roles of *being a participant* and *being an observer* (Skjervheim 1996). These roles typify countervailing ways by which we relate to human performances and practices.

[79] This is not saying that Quine and Williamson would have approved of the present project. Frankly, I am unsure how they would respond to it.

By relating to a performance as a participant, we assume the same stance as the person who executed the performance. We treat the performance as subject to normative assessment; as something that can be right or wrong. If the performance in question is an utterance, for instance, we treat its content as something we—the consumers of the utterance—are considering whether we should hold or not. In short, we treat it as a claim. By contrast, in the role of being an observer of the very same utterance, we are treating it as a factual occurrence. In that perspective, the content is not subjected to normative assessment. Instead, the utterance is treated as any other kind of occurrence; as a fact we perhaps could have an interest in giving a causal explanation. Our focus in the first role is the content of the claim; a focus we share with the claim's author. Our focus in the second role is *that* a claim with a certain content was made.

In our role as participants of a discursive practice, we apply concepts in judgment. We make claims, infer the consequences of these, give explanations and explications, attempt to understand things, try to gain more knowledge about things, and so on. Moreover, we subject these activities to normative assessment. For instance, we consider whether a particular claim is true and whether an explanation is good. By claiming that conceptual analyses are constrained rationally and pragmatically, I am claiming that our interests as users of the concepts we analyze are not decoupled from us as analysts of those concepts. To the contrary, these "first order" interests are what substantiate the norms. This is seen most clearly with the norm for explanatory and expressive utility. Holding it involves holding that analyses are better when they benefit our discursive activities. The other norms are also substantiated by concerns that only users of the concept can have. For instance, (IC) requires that concepts do not have contents for which applying the concept would imply a contradiction. From the point of view of an observer of a discursive practice, there is no reason to hold such a norm. If practitioners are applying a concept that cannot be applied truthfully, that would count as a remarkable fact from this point of view, but it be no miracle; no violation of a law of nature.

So our practical interests with a concept have a bearing on the goodness of its analyses. This comes to the fore when we ask: What is the value of licensing an inference from the obtainment of certain conditions, to the applicability of the concept

(and vice versa)? What are we trying to achieve by making claims that have this significance? We can give plausible answers to these questions if we take the rational and pragmatic dimensions of discursive practice into account. For instance, the value of licensing such inferences is clear when the obtainment of an analysis' ACs is a determinate matter. If the ACs are determinate, we can sharply distinguish the instances to which the concept is applicable and those to which it is not. It is only through our first order dealings with the concepts we analyze that we can appreciate the need for sharp distinctions. For instance, a determinate distinction between intentional action and non-intentional action is decisive for most justice systems. If INTENTIONAL ACTION were not determinate, courts would issue arbitrary rulings. A just society cannot sustain arbitrary rulings. Hence, our practical interests with the concept of an intentional action constrain analyses of that concept in a straightforward and definite way.

Contrast with an empirical-psychological perspective on discursive practice. Psychologists seize the role as observers of that practice. For instance, Rosch and Mervis (1998) study how people in fact categorize items. Their claim is that categories (read: concepts) exhibit "family resemblance structures." According to their view, the structure of categories is not *criterial*; which is how we treat concepts when giving them AC-based analyses. Instead, a category is "held together" by family resemblances between the members of the category. The status of their claim is more transparent upon seeing how they justify it. In the experiments they have conducted, subjects express the attributes they associate with a number of items within a category. For VEHICLE, for instance, the subjects list the attributes they associate with CAR, MOTORCYCLE, AIRPLANE, TRACTOR, WHEELCHAIR, ELEVATOR, etc. (Rosch and Mervis 1998: 22). The superordinate category exhibits a family resemblance structure if the items within it have a differing number of attributes they share with the other members, and if there are few or no attributes common to all the members. The claim that categories have a family resemblance structure is, in this way, true or false pending what the data shows. Apart from discrediting some attributions that are "clearly and obviously false" according to a pair of judges (Rosch and Mervis 1998: 21), their claim is completely subjected to how the experimental subjects in fact proceed to classify the items. Hence, the claim that categories exhibit a family resemblance structure is not a normative claim. They are not treating the subjects'

attributions as right or wrong; as purporting to determine how we ought to employ the respective categories. Rosch and Mervis are not in the errand of displaying the best or most useful structure of a concept. Clearly, conditioning the applicability of a concept on a loosely circumscribed set of attributes—attributes that are collectively insufficient, and none of which are necessary—is not a good representation of the concept for purposes of deciding whether it is correctly applicable on a particular occasion.

Demonstrating that a psychological theory is unfit for some purposes is not demonstrating that the theory is not valuable on its own merits. Gaining knowledge about how concepts in fact are employed and how they are actually grasped by their users are interesting pursuits in their own right. But these are not the interests conceptual analyses serve. Philosophers are not simply doing a poorer job at theorizing concepts than psychologists. They are answering a different set of questions. I have highlighted the distinctiveness of that set of questions by associating it with the role of being a participant.

4.4 The concepts are ours

In chapter 2 I argued that intuitions are cognitive pro-attitudes—toward propositional contents—with two peculiar traits. These traits gave us no additional reason to suppose that intuitions are evidence. Since there is no reason to assign evidential status to cognitive pro-attitudes in general, intuitions were denied evidential status. In that way, chapter 2 left us with nothing special to say about philosophical method. Chapter 3 was an attempt to make good on this void. It elaborated and defended a handful of norms for conceptual analysis. On the resulting account, an analysis' goodness is determined by multiple factors, of which only one has been sufficiently appreciated in the methodological debate. The present chapter has elaborated this notion of good analysis, and linked it to the role of being a participant of a discursive practice.

Surely, the title "Concepts Unbound" is somewhat paradoxical. If I have shown anything, it must be that conceptual analyses are bound in a host of different ways. Therefore, if we hold that conceptual analyses purport to determine the contents of the analyzed concepts, those concepts are themselves bound in a host of different ways. However, they are only bound by us. The purposes we have as participants of a

discursive practice, and the norms these substantiate for conceptual analyses, are all *our* purposes and norms. Moreover, the analyzed concepts are *our* concepts. This does not, of course, mean that we can construe them in arbitrary ways. Quite the contrary. The ways in which we correctly constrain analyses are not imposed on us from some source beyond the discursive practice we participate in. That does not make these norms "subjective" in the bad sense.

The things we assume responsibility *to*, in performing conceptual analysis, are also things we have responsibility *for*. This is the sense in which the concepts we analyze are *ours*, and the sense in which we are *participants* of a discursive practice as analysts. It is up to us to maintain and improve our conceptual resources. There is a ubiquitous danger of leaving out the responsibility-*for* part when philosophy is building its own image in the presence of modern science. It is tempting to model philosophy's epistemology and methodology on that of modern science. But philosophy is not an empirical science. We are not observers of the practices we theorize. If the main contention of this thesis is right, we are participants in the Skjervheimian sense, and that role has a distinctively normative dimension.

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